PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From January 10, 1856 *to June* 18, 1857 *inclusive.*

(BEING A CONTINUATION OF THE SERIES ENTITLED
"ABSTRACTS OF THE PAPERS COMMUNICATED TO
THE ROYAL SOCIETY OF LONDON.")

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Page 394, line 6 from bottom, *dele* half.
— 398, — 4, *for* 2,000,000 *read* 5200×10^6; and *for* 8,200,000 *read* 21000×10^6.
— 398, — 10, *for* 10 tons *read* 53600×10^6 tons; and *for* 42 tons *read* 874000×10^6 tons.

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— 51, — 8, *for* property *read* quality.
— 53, — 8, *for* a given result *read* given a result.
— 54, — 12, *for* that *read* steel.
— 125, — 11, *for* $\sin\theta$ *read* $\cos\theta$.
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— 157, — 10, *for* no *read* or without.
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— 182, — 2, *for* 268° *read* 238°.
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— 309, — 5, *for* Θ' *read* Θ'^n.
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— 488, — 12 from bottom, *for* June 15 *read* June 18.
— 488, — 3 „ „ *for* M.D. *read* Esq.
— 573, — 11, *for* P(1, 2, 3, 4, 5, 6,) 9 *read* P(1, 2, 3, 4, 5, 6)q.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

January 10, 1856.

ADMIRAL BEECHEY, V.P., in the Chair.

In consequence of there not being a sufficient number of Fellows present, the Ballot for the question of the readmission of Mr. Sievier was postponed to the next Meeting.

The following communications were read:—

I. "On Insolinic Acid." By AUGUSTUS W. HOFMANN, Ph.D., F.R.S. &c. Received December 20, 1855.

(Abstract.)

In attempting to purify cuminic acid by boiling with chromic acid, I observed that this acid experienced, on the part of this reagent, a progressive alteration. By twenty-four hours' ebullition, cuminic acid is completely converted into an acid insoluble in alcohol

and ether, for which I propose the provisional name of *insolinic acid*; purified by the ordinary processes, this body furnished on analysis the following relations:—

$$C_9 H_4 O_4;$$

but the analysis of the salts demonstrates that this formula must be doubled, insolinic acid being a bibasic acid.

I have examined the following salts:—

Insolinic acid	$C_{18} H_8 \quad O_8$
Silver salt	$C_{18} (H_6 Ag_2) \; O_8$
Copper salt..................	$C_{18} (H_6 Cu_2) \; O_8$
Barium salt	$C_{18} (H_6 Ba_2) \; O_8$
Calcium salt (at 100° C.)	$C_{18} (H_6 Ca_2) \; O_8$
,, (at 133° C.)	$C_{18} (H_6 Ca_2) \; O_8$
Potassium salt (neutral)	$C_{18} (H_6 K_2) \; O_8$
,, (acid)..........	$C_{18} (H_7 K) \; O_8$
Potassium-sodium salt	$C_{18} (H_6 K Na) \; O_8$

When considered by itself, insolinic acid has but slight claims on the attention of chemists; but when viewed in connexion with other groups of bodies, it acquires increased interest. Some years since, Gerhardt pointed out that to the homologous series of monobasic fatty acids $C_{n2} H_{n2} O_4$, the lowest terms of which are formic and acetic acids, there runs parallel a homologous series of bibasic acids, $C_{n2} H_{n2-2} O_8$, the simplest member of which is oxalic acid. These two series of acids are connected by the closest ties, and very conclusive experiments have demonstrated that the members of the former may be easily converted into those of the latter; such is the case of the transformation of butyric into succinic acid, effected by M. Dessaignes under the influence of oxidizing agents.

The following table exhibits these two series of acids arranged according to their carbon:—

Formic acid	$C_2 \; H_2 \; O_4$
Acetic acid............	$C_4 \; H_4 \; O_4$
Propionic acid	$C_6 \; H_6 \; O_4$
Butyric acid	$C_8 \; H_8 \; O_4$
Valeric acid	$C_{10} H_{10} O_4$
Caproic acid	$C_{12} H_{12} O_4$

Œnanthylic acid	$C_{14} H_{14} O_4$
Caprylic acid..........	$C_{16} H_{16} O_4$
Pelargonic acid	$C_{18} H_{18} O_4$
Rutic acid	$C_{20} H_{20} O_4$
Oxalic acid............	$C_4 H_2 O_8$
——?	$C_6 H_4 O_8$
Succinic acid	$C_8 H_6 O_8$
Pyrotartaric acid	$C_{10} H_8 O_8$
Adipic acid	$C_{12} H_{10} O_8$
Pimelic acid	$C_{14} H_{12} O_8$
Suberic acid	$C_{16} H_{14} O_8$
——?	$C_{18} H_{16} O_8$
Sebacic acid	$C_{20} H_{18} O_8$

The existence and the mode of formation of insolinic acid prove that to the series of monobasic aromatic acids, $C_{n2} H_{n2-8} O_4$, the lowest known term of which is benzoic acid, there corresponds likewise a series of bibasic acids, $C_{n2} H_{n2-8-2} O_8 = C_{n2} H_{n2-10} O_8$. Of this series few members are at present known, but the group of aromatic acids is itself very imperfect and limited. The two series comprise at present the following terms:—

Benzoic acid	$C_{14} H_6 O_4$
Toluylic acid..........	$C_{16} H_8 O_4$
——?	$C_{18} H_{10} O_4$
Cuminic acid..........	$C_{20} H_{12} O_4$
——?	$C_{14} H_4 O_8$
Terephthalic acid ...... / Phthalic acid..........	$C_{16} H_6 O_8$
Insolinic acid..........	$C_{18} H_8 O_8$
——?	$C_{20} H_{10} O_8$

If we take the carbon as the standard of comparison, it is evident that the bibasic insolinic acid corresponds to the monobasic acid, which stands between toluylic and cuminic acid. In addition to this unknown acid, toluylic acid only is represented in the series of bibasic acids. There are, in fact, two bodies which may be regarded as representatives of toluylic acid, namely, phthalic and terephthalic acids. Benzoic and cuminic acid are not yet represented.

II. "On the Existence of Multiple Proportion in the quantities of heat, or equivalent alteration of internal space of bodies, caused by definite changes of state as produced by Chemical Combination or otherwise." By THOMAS WOODS, M.D. Communicated by Prof. STOKES, Sec. R.S. Received November 28, 1855.

(Abstract.)

Gay-Lussac having shown that the combining proportions of gases and vapours are either equal in volume or some multiple of each other, and other chemists, particularly Playfair and Joule, having extended the same law to solids and liquids, it is evident that *specific volume* of its combining equivalent is characteristic of matter. But as every substance is composed of matter and space, or of particles with some distance between them, as is shown by expansion and contraction, whenever volume is altered there must be either an addition to or subtraction from the internal space of the body. This alteration of volume is evident in the case of bodies expanding or contracting by gain or loss of heat; but in chemical combination, where alteration of internal space must take place also (as shown by change of temperature, and because *specific* volume being characteristic of matter, this volume must change when the matter changes, by a substitution of a mixture of two kinds of matter for one), in chemical combination, I say, this alteration of internal space is not so plainly demonstrable. Still, in the change of temperature, we have not only an evidence, but a measure of the change of state of combining bodies. For, the phenomena of heat being produced by a *dual* force acting equally in opposite directions, one body cooling or contracting as another becomes heated or expands, it may be taken for granted that whenever heat or expansion is found in one body, the opposite change is occurring in some other. Now, regarding this proposition as true, it is intended to be proved from the combinations of oxygen that the internal space of a substance is lost and gained in multiple proportion, in the definite changes of state of bodies, such as in the condensation of vapours into liquids, and liquids into solids, and the reverse; and also in chemical combi-

nations and decompositions; and therefore that the *space* as well as the *matter* of which volume is composed can be only added to or taken away in what is called its combining equivalent.

In order to find whether the heat of chemical combination, which is taken as equivalent to the alteration of internal space, is equally produced by the same substance uniting with others, or if not, if it is given out in multiple proportion, oxygen is made to combine with several other simple bodies, and the alteration of temperature noted.

The method of oxidizing these substances, the details of each process being given in the paper, consisted in dissolving them in some suitable menstruum,—for instance, in sulphuric acid, liquor of potass, and nitric acid. When the two former are used, water is decomposed to oxidize the dissolved body; in the last case, the nitric acid is resolved into oxygen and binoxide of nitrogen, the former of which unites with the substance to be oxidized, the latter escaping. Other combinations and decompositions at the same time take place (as detailed in the paper), and being taken into account (decompositions absorbing as much heat as is produced by the combination of the constituents), the alteration of temperature by the oxidation alone is arrived at.

In this manner eighteen different metals were oxidized, but the heat of oxidation was obtained satisfactorily only with twelve. Other experimenters (Favre and Silbermann and Andrews) have, with a different object in view, found the heat of oxidation of fourteen other substances; their conclusions are added as being from unprejudiced sources, and the result of all the experiments is brought together in a table, in order to see whether the law of multiple proportion exists. The numbers found by the different experimenters are all calculated to the same standard. The unit of heat is the amount necessary to raise the temperature of 1000 grains of water 1° Fahr., and the quantity of the metal oxidized is an equivalent of each, oxygen = 1.

To find whether the law extends to change of state when no chemical combination takes place, the amount of heat given out by the condensation of an equivalent of steam, and by the solidification of an equivalent of water, is given. The following is the table giving the thermal equivalents of the several substances, the names of the experimenters, and the ratio of proportion. It is to be remarked,

that the condensation of steam being in multiple proportion with the other thermal equivalents, the expansion of all other bodies into vapour must be included; for I showed in the Philosophical Magazine for January 1852 (page 48), that all bodies expand into vapour in some multiple of their atomic volumes, and their atomic volumes being in ratio also, their expansion or gain of internal space in this definite change must be in multiple proportion.

TABLE showing the quantity of heat produced in 1000 grs. of water by the oxidation of an equivalent of each substance, O=1.

Name of substance oxidized.	Units of heat.	Ratio.	Name of Experimenter.
Latent heat of ice ...	·1603	·1603	
Latent heat of steam	1·287	8 times ·1603	
Iodine.................	·8	5 times ·1603	Woods.
Chlorine..............	−1·6	....	Favre and Silbermann.
Nitrogen..............	1·6	twice ·8	Woods.
Silver	1·6	....	Woods.
Selenium	2·7	....	Favre and Silbermann.
Mercury..............	2·4	3 times ·8	Woods.
Palladium	2·42	....	Woods.
Molybdenum	3·38	4 times ·8	Woods.
Carbon	3·3		Favre and Silbermann.
Arsenic	4·8	....	Favre and Silbermann.
Antimony	4·8	....	Woods.
Copper	4·9	6 times ·8	Favre and Silbermann.
Cobalt	4·8	....	Woods.
Bismuth..............	4·82	....	Woods.
Nickel................	6·5	8 times ·8	Woods.
Lead	6·2		Favre and Silbermann.
Hydrogen	7·8	....	Favre and Silbermann.
Tin	8·0	....	Mean of Andrews and Favre and Silbermann.
Phosphorus	8·1	10 times ·8	Favre and Silbermann.
Cadmium	8·18	....	Woods.
Iron	7·95	....	Mean of Andrews and Favre and Silbermann.
Zinc	9·6	12 times ·8	Favre and Silbermann.
Manganese............	10·4	13 times ·8	Woods.
Barium	12·8	16 times ·8	Woods.
Aluminium............	16·16	20 times ·8	Woods.
Sodium	17·5	22 times ·8	Favre and Silbermann.
Potassium	17·3		Favre and Silbermann.

Note.—I proved in a paper published in the Philosophical Magazine for October 1851, that "the decomposition of a compound body absorbs as much heat as the combination of the elements originally

produced." I believe I was the first to prove this as a general proposition, and, by so doing, laid the foundation of almost all the thermo-chemical researches since carried on; for, as far as I am aware, no process which took decomposition into account was used before my paper was published.

In a paper read to the British Association at Belfast, and published in the Philosophical Magazine for November 1852, I proved that the intensity of chemical affinity might be measured by the quantity of heat produced by the combination.

As regards the first of these papers, Mr. Joule published in the Philosophical Magazine for June 1852, a memoir proving exactly the same proposition, but giving me the merit of priority in a preliminary remark. It is, however, singular that Favre and Silbermann bring forward in 1853 (Annales de Chimie et Physique, vol. xxxvii. p. 507) the very same experiments to prove the same fact, and give it as their own.

As regards the second paper. In six months after its publication, Messrs. Favre and Silbermann (Annales de Chimie et Physique, vol. xxxvii. p. 484) prove the same truth with the same experiments, using exactly the same metals, and give their memoir as producing an original idea.

I notice these coincidences here as being remarkable, and because the propositions contained in the paper referred to are the groundwork of the present experiments, and also with a view to prevent an unconscious repetition on the part of Messrs. Favre and Silbermann.

January 17, 1856.

Professor WILLIAM ALLEN MILLER, M.D., V.P., in the Chair.

The question of Mr. Sievier's readmission into the Society was put to the Ballot, and the Ballot having been taken, Mr. Sievier was declared to be readmitted.

The following communications were read:—

I. "Anatomical and Physiological Observations on the Organ of Vision, for the purpose of ascertaining the alterations caused by section of the Optic Nerve." By AUGUSTUS WALLER, M.D., F.R.S. Part I. Received January 10, 1856.

(Abstract.)

The author begins by referring to experimental researches already published by him on the effects produced on the nerves of living animals by section at different points in their course; and he briefly recapitulates the chief results of these experiments, as follows, viz.—1. Section of a spinal nerve on the distal side of its ganglion is followed by degeneration of the part of the nerve beyond the section, whilst the part still connected with the ganglion retains its integrity. 2. Section of the posterior root of a spinal nerve induces no change in the trunk or branches of the nerve beyond the ganglion, nor in the portion of the root which remains connected with the ganglion, whereas the part of the root attached to the spinal cord, but disconnected with the ganglion, becomes disorganized. His conclusion from these experiments, which have been varied in different ways, is "that the spinal ganglion is the centre of the nutritive power of the nerve-fibres adherent to it." From other experiments, he concluded that the anterior spinal roots derive their nutritive influence from the spinal cord; and he has little doubt "that every nerve-fibre is in

connexion with a nerve-cell, which exerts over it a peculiar influence which enables it to retain its structure," &c. He does not think it proved, however, that all ganglia exert an influence both in a central and peripheral direction like those of the posterior spinal roots, and indeed experiments have shown that certain ganglia, such as the superior cervical of the sympathetic, exert their sustaining power on the nerve-fibres connected with them, in one direction only.

The investigation of the effects of section on the optic nerve, which forms the subject of the present communication, promised to yield interesting results on several grounds, and especially as calculated to throw light on the relations of the fibres of the nerve to those of the opposite one in the optic chiasma or commissure, and on the question as to the manner in which they are connected with the brain.

After some observations on the structure of the optic nerve, the author describes his experimental procedure. To see the nerve whilst operating, and thus be assured that the section was effected at the desired point, he began by dislocating the eyeball forwards from its socket, while the animal was under the influence of ether. This operation he found could be effected in the rabbit, by simply pressing the eyelids widely apart, and was unattended with any serious disturbance of the functions of the organ. In dogs the operation is more difficult, and occasions more injurious effects. When the eye is dislocated, its axis is inclined downwards and inwards, and the optic nerve can be reached without difficulty; so that it may readily be cut at any desirable distance behind its place of entrance into the eyeball, and may even by moderate traction be broken off immediately before the chiasma.

After stating various observations which he made on the dislocated eye, the author gives an account of the effects which resulted from section of the optic nerve. The condition of the retina after the operation, was studied during the life of the animal by means of the ophthalmoscope; and, after death, its structure, as well as that of the optic nerve before and behind the place of section, the chiasma, the optic tracts, and connected part of the brain, was examined with the microscope.

The elements of the retina, as well as those of Jacob's membrane, were found unaltered four months after the time of section. The

distal part of the optic nerve (that left in connexion with the eyeball), examined after the lapse of a month in one case, and of two months in another, was also discovered to be unaltered. On the other hand, the part of the nerve behind or on the central side of the section was invariably disorganized. The section was usually performed on the optic nerve of the right eye, and the disorganized fibres of its central segment could be traced back to the left optic tract, through the chiasma, where they obviously decussated with the sound fibres of the opposite nerve. The right optic tract had undergone no change; the fibres of the left tract were disorganized as far back as the quadrigeminate bodies, except those running along the posterior or inner border of the tract; which exception appears to the author to favour the opinion that fibres pass along the tracts and commissures from the quadrigeminate bodies of one side to those of the other side, without connecting themselves with the retina. On the other hand, the results of his experiments do not seem to him to countenance the notion of fibres running in the optic nerves from one retina to the other without connexion with the brain, nor the generally received doctrine that part of the fibres of the optic nerve are continuous with the optic tract of the same side; on the contrary, the whole fibres of the nerve would seem to undergo decussation.

The microscopic characters of the atrophied and disorganized nervous substance are described in the paper; they were found to differ somewhat in the part of the nerve before and that behind the chiasma, owing no doubt to the different structure of these parts in the sound state.

The changes produced in the geniculate and quadrigeminal bodies will be communicated in the succeeding part of the paper.

II. "On some of the Metamorphoses of Naphthalamine." By A. W. Hofmann, Ph.D., F.R.S. &c. Received January 10, 1856.

The great facility with which some of the nitro-hydrocarbons can be reduced by means of iron and acetic acid—the modification of Zinin's process, lately proposed by M. Béchamp—enables us to

obtain the corresponding bases in larger quantity, and to examine their derivatives more minutely.

One of the bases, for the preparation of which this process is particularly applicable, is naphthalidine, or as it is more appropriately called, naphthalamine. Mr. William H. Perkin is engaged in examining the deportment of this substance with chloride of cyanogen, and the following is a summary of the results he has at present obtained.

Fused naphthalamine, when submitted to the action of chloride of cyanogen, absorbs this gas with great avidity, and is gradually converted into a dark resinous mass. This is the hydrochlorate of a new base, which has received the name of menaphthalamine, in consequence of the analogy of its origin with that of melaniline, derived by a similar process from aniline.

Menaphthalamine is separated from the hydrochlorate by potassa, washed and repeatedly recrystallized from alcohol. It contains

$$C_{42}H_{17}N_3,$$

nd is formed according to the equation

$$\underbrace{2C_{20}H_9N}_{\text{Naphthalamine.}} + \underbrace{C_2NCl}_{\text{Chloride of cyanogen.}} = \underbrace{C_{42}H_{17}N_3HCl.}_{\text{Hydrochlorate of menaphthalamine.}}$$

Mr. Perkin has verified this formula of menaphthalamine by the analysis of the hydrochlorate and of the platinum salt, which respectively contain

$$C_{42}H_{17}N_3, HCl \text{ and}$$
$$C_{42}H_{17}N_3, HCl, PtCl_2.$$

Among the various metamorphoses which menaphthalamine undergoes under the influence of agents, the deportment of this substance with cyanogen has especially engaged the attention of Mr. Perkin.

Menaphthalamine, like melaniline, absorbs two equivalents of cyanogen, and is converted into a slightly crystalline buff-coloured substance, which retains feebly basic properties.

The analysis of this body, which, from its composition, may be termed dicymenaphthalamine, has led to the formula

$$C_{46}H_{17}N_5 = C_{42}H_{17}N_3 + Cy_2.$$

Dicymenaphthalamine is insoluble in water, moderately soluble in alcohol and ether. It dissolves readily in acids. The latter solution, when precipitated by potassa, immediately after it has been made, yields unchanged dicymenaphthalamine; but if the solution be allowed to stand for a few moments, a yellow substance is precipitated, which is no longer a salt of dicymenaphthalamine.

The composition of this yellow body, which, in accordance with the terminology adopted in the aniline series, may be called menaphthoximide, is represented by the following formula—

$$C_{46}H_{15}N_3O_4,$$

and its formation is illustrated by the equation—

$$\underbrace{C_{46}H_{17}N_5}_{\text{Dicymenaphthalamine.}} + 4HO + 2HCl = \underbrace{C_{46}H_{15}N_3O_4}_{\text{Menaphthoximide.}} + 2NH_4Cl.$$

In fact, the mother-liquor of this substance contains a large amount of ammonia.

Menaphthoximide may be viewed as binoxalate of menaphthalamine *minus* 4 equivalents of water—

$$\underbrace{C_{42}H_{17}N_3HC_2O_4,\ HC_2O_4}_{\text{Binoxalate of menaphthalamine.}} - 4HO = \underbrace{C_{46}H_{15}N_3O_4}_{\text{Menaphthoximide.}};$$

and this view is corroborated by the deportment of the substance with potassa, which reproduces menaphthalamine and oxalic acid.

From the preceding experiments, it is obvious that the deportment of naphthalamine with chloride of cyanogen is perfectly analogous to that of aniline. The subsequent metamorphoses of the newly-formed compound also exhibit the same analogy.

Aniline series.		Naphthalamine series.	
Aniline......	$C_{12}H_7N$	Naphthalamine	$C_{20}H_9N$
Melaniline ..	$C_{26}H_{13}N_3$	Menaphthalamine ..	$C_{42}H_{17}N_3$
Dicymelaniline	$C_{30}H_{13}N_5$	Dicymenaphthalamine	$C_{46}H_{17}N_5$
Melanoximide	$C_{30}H_{13}N_3O_4$	Menaphthoximide ..	$C_{46}H_{17}N_3O_4$

Results of much interest are to be expected from the examination of the products formed by the action of heat on menaphthoximide.

It is probable that this reaction would produce the naphthalamine term, corresponding to anilocyanic and cyanic acid.

Cyanic acid..............	$C_2\ H\ NO_2$
Anilocyanic acid..........	$C_{14}\ H_6\ NO_2$
Naphthocyanic acid	$C_{22}\ H_8\ NO_2$

Menaphthoximide, when heated, yields, in fact, a vapour of a most penetrating organic odour; but Mr. Perkin has not yet obtained sufficient material for a more minute examination of the body to which it belongs.

January 24, 1856.

Sir BENJAMIN BRODIE, Bart., V.P., in the Chair.

A paper was in part read, entitled "Account of Pendulum Experiments undertaken in the Harton Colliery for the purpose of determining the Mean Density of the Earth." By G. B. Airy, Esq., Astronomer Royal. Received December 26, 1855.

January 31, 1856.

The LORD WROTTESLEY, President, in the Chair.

The reading of Mr. Airy's paper, entitled "Account of Pendulum Experiments undertaken in the Harton Colliery for the purpose of determining the Mean Density of the Earth," was resumed and concluded.

(Abstract.)

In the first section of this paper, the author explains the reasons, founded on calculation, which appeared to make it probable that the comparison of gravity at the top and the bottom of a mine

would give means of determining the earth's mean density with accuracy, perhaps superior to that obtained in the Schehallien or the Cavendish experiment; and which induced him first in the summer of 1826 (in concert with Dr. Whewell), and again in 1828 (with Dr. Whewell, Mr. Sheepshanks and others), to try the experiment in the Dolcoath mine near Camborne in Cornwall. These attempts were both frustrated by accidents having no connexion with the essential parts of the experiment. After a lapse of many years, he found that several circumstances (of which one was the general familiarity with the manipulation of the galvanic telegraph and the facility of applying it to the comparison of widely separated clocks) were very favourable to a repetition of the experiment; and having selected the Harton Colliery in the neighbourhood of South Shields as a fit place, in which two stations could be found in exactly the same vertical but at 1256 feet difference of height, and being assured of every assistance from the owners of the mine, he proceeded with the experiments in the months of September and October 1854.

The principal instruments employed were two detached pendulums on iron stands, the property of the Royal Society, which were most carefully repaired by Mr. Simms; graduated arcs, barometers, thermometers, &c.; two clocks, one the property of the Royal Society, which were fitted for this purpose with inclined gilded reflectors upon the pendulum bobs, intended to be illuminated by the light of lamps passing through holes in the side of the clock-cases; galvanometer-needles attached to the clock-cases, with circuit-breakers; a galvanic battery at the upper station; a journeyman-clock at the upper station, fitted with an apparatus by which it completed the galvanic circuit at every 15^s of its own time; and two galvanic wires passing down the mine-shaft and forming a closed circuit through the battery, the journeyman-clock, and the two galvanometers.

The working party consisted of Mr. Dunkin (superintendent) and Mr. Ellis from the Royal Observatory, Mr. Pogson from the Observatory of Oxford, Mr. Creswick from the Observatory of Cambridge, Mr. G. Rümker from the Observatory of Durham, and Mr. Simmonds from Mr. Carrington's Red Hill Observatory.

The plan of operations was this. Simultaneous observations of the two pendulums (one in the upper and the other in the lower

station) were kept up *incessantly* during the whole working time (day and night) of one week; then the pendulums were interchanged and were observed in the same manner through another week; after this the pendulums were twice interchanged, but the two last series of observations were so much shortened that both were included in one week. Each pendulum had six swings of nearly four hours each, on every day of observation; and between the end of one swing and the beginning of the next, numerous galvanic signals were passed for the comparison of the clocks.

The second section gives the details (as far as space permits) of the comparisons of clocks by the galvanic signals. On examining the proportion of rates, it was found that there was distinctly a personal equation in the observation of the galvanic signals. Approximate values for the different observers were obtained, and the proportion of rates was corrected (where necessary) for these equations.

The third section describes the general system of observing the pendulums and reducing the observations. For ascertaining the time of a coincidence of the vibration of the detached pendulum with that of the clock pendulum, the mean of the times of the first disappearance and the last re-appearance was employed. Several coincidences were observed at the beginning of a swing and the mean was taken: and several were observed, and the mean taken, at the end of the swing. From these means, a mean interval of coincidences was obtained; from which the ratio of the actual rate of the detached pendulum to that of the clock pendulum was found. This requires several corrections.

The correction depending on the arc of vibration, with no data except the first and last arcs of vibration, and no assumption of a mathematical law for the intermediate arcs, is made to depend on the results of experimental observations on the numerical decrease of the arc by a peculiar process.

The corrections depending on the temperature and the atmospheric pressure are based mainly on Sabine's experiments.

The fourth section contains an abstract of the Pendulum Observations at the Upper Station, with the corrected logarithm of the rate of the detached pendulum on the clock pendulum for every swing; and the fifth section contains a similar abstract for the Lower Station.

The sixth section gives the computation of the logarithm of the rate of the lower detached pendulum upon the upper detached pendulum (for which the preceding sections have furnished the elements). Then is given in detail the investigation, by the Theory of Probabilities, of the formula for the best combination of the results of the different swings. The advantage of the method of incessant observations with numerous comparisons of the clocks is pointed out. The formula is applied to the four series of observations; and the results of the first and third series agree very closely, and those of the second and fourth series agree very closely, showing that the pendulums had undergone no sensible change. By comparing the mean of the first and third series with the mean of the second and fourth, the proportion of pendulum rates at the upper and lower stations is obtained independently of the pendulums employed. The conclusion is that gravity below is greater than gravity above by $\frac{1}{19286}$th part, with an uncertainty of $\frac{1}{270}$th part of the excess; or that the acceleration of a seconds' pendulum below is $2^s{\cdot}24$ per day, with an uncertainty of less than $0^s{\cdot}01$. Reasons however are given for believing that the uncertainty is greater than this quantity.

The seventh section contains a description of the operation for measuring the depth of the mine. It then treats of the process to be employed for computing the proportion of gravity at the upper and lower stations (without reference to the experiments), on an assumed proportion of the density of the mine-rocks to the earth's mean density. It is shown that, supposing the upper surface of the ground about Harton to have the true spheroidal form, it is unnecessary to give any attention to the irregularities of the surface on distant parts of the earth. It is also shown that there is no reason to doubt the correctness of the law of decrease of the attraction of the earth's nucleus as depending on the elevation of the station, unless there be some serious irregularity in the arrangement or density of the matter immediately below Harton. Assuming this to be insensible, the theory of correction for the inequalities of ground in the neighbourhood of Harton is then considered. The elevation of the upper station is about 74 feet above high water; and as it appears from this that the depth of inequality can in no case amount to one-tenth of the depth of the lower station, it is easily found that

the excess or defect of attraction will be computed with sufficient accuracy by supposing the excess or defect of matter to exist absolutely at the surface; in which case the effect on the upper station is nothing, and that on the lower station is easily computed. For depressions like that of the sea bounded (at least for the purposes of computation) by a straight line near the mine, but unlimited in the other direction, a simple formula is found.

For the application of these theorems it was necessary to have a map giving the elevations of the ground at various points. By instruction of the Mayor and Corporation of South Shields, the Corporation Surveyor, Christopher Thompson, Esq., prepared such a map. In the use of it, it was found convenient to adopt as unit of linear measure the depth of the mine. A line at the distance of ten depths very nearly touches the cliffs of Tynemouth, of Frenchman's Point, and of some points further to the south-east. The land generally is divided into squares whose sides are one depth each, and these are grouped as appears convenient for representing approximately the form of the ground by compartments each of a uniform elevation through its extent. The principal requirements are, besides taking account of the depression of the sea beyond the ten-depth line, to estimate the effect of the curvature of the coast towards the mouth of the Wear, to compute the effect of the hollow of Jarrow Slake, and generally to make proper allowance for the absence of matter in the valley of the Tyne. There are also some small elevations to be considered. The general result is, that the attraction of the regular shell of matter is to be diminished by about $\frac{1}{280}$th part.

Putting D for the mean density of the earth, d for that of the shell, the fraction $\dfrac{\text{Gravity below}}{\text{Gravity above}}$ is computed to be $1{\cdot}00012032 - 0{\cdot}00017984 \times \dfrac{d}{\mathrm{D}}$. The pendulum experiments give 1·00005185. The comparison of these gives $\dfrac{\mathrm{D}}{d} = 2{\cdot}6266$.

The eighth section contains a detailed account of the strata passed through in sinking the Harton shaft, and the specific gravities of many of the beds as determined by Professor W. H. Miller. The result for the mean specific gravity is 2·50. Substituting this in the

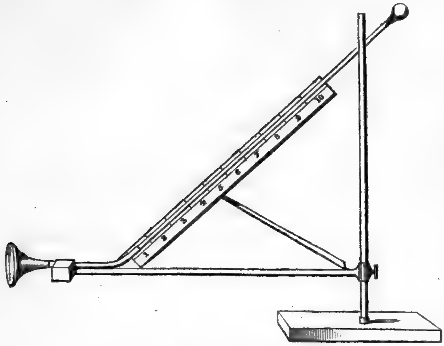
equation given by the pendulum experiments, the mean specific gravity of the earth is found to be 6·566. Adverting to the excess of this number above those given by the Schehallien and the Torsion-rod experiments, the author remarks that it is very difficult to assign the causes or the measures of error in either of the experiments, but expresses his belief that the result of the present experiment may compete on at least equal terms with the others.

A paper was also read, entitled "A Description of a new Sphygmoscope, an Instrument for indicating the Movements of the Heart and Blood-vessels; with an Account of Observations obtained by the aid of that Instrument." By S. Scott Alison, M.D., Licentiate of the Royal College of Physicians, London. Communicated by G. O. Rees, M.D., F.R.S. Received January 12, 1856.

The sphygmoscope (fig. 1) consists of a small chamber containing spirits of wine or other liquid, provided with a thin india-rubber wall, where it is to be applied to the chest. At the opposite extremity the chamber communicates with a glass tube, which rises to some height above the level of the chamber. Liquid is supplied to the instrument until it stands in the tube a little above the level of the chamber. The pressure of the column of liquid in the tube acts upon the elastic or yielding wall of india-rubber and causes it to protrude. This protruding part or chest-piece is very readily affected by external impulse; it yields to the slightest touch, and being pushed inwards, causes a displacement of the liquid in the non-elastic chamber, and forces a portion of liquid up the tube. The protruding wall of india-rubber is driven inwards when it is brought in contact with that portion of the chest which is struck by the apex of the heart, and a rise in the tube takes place. When the heart retires, the india-rubber wall, affected by the pressure of the column of liquid in the tube, is pressed back, follows the chest, and permits the liquid to descend. The degree to which the india-rubber wall is forced in by the apex of the heart is denoted by a corresponding rise in the tube, and the amount of protrusion of the india-rubber

wall which takes place when the heart retires is denoted by a corresponding fall in the tube. The tube is supplied with a graduated

Fig. 1.



Sphygmoscope.

scale to denote the rise and fall with exactitude. The glass tube is provided at the top with some contrivance, such as a brass screw and collar, to prevent the egress of the liquid when the instrument is not in use, or a bulb with an orifice may be supplied. When employed, the glass tube is left open to permit of the passage of the air to and fro.

The sphygmoscope is mounted upon a stand. The chamber and tube are fitted to a horizontal arm, which is made to move up and down so as to carry the instrument to the desired height. The base is so made as to secure the requisite immobility.

The glass tube is a foot or more long, and the round bore is about the one-eighth part of an inch. If the bore be much larger, the movement will be inconsiderable ; if much less, capillary attraction will interfere and prevent free motion.

When the instrument is to be employed, mounted upon its stand, it is placed upon a firm table with the chamber projecting beyond it. The person whose heart is to be examined is seated upon a firm chair, with his chest erect and free from motion. The protruding india-rubber wall of the chamber or chest-piece is delicately made to touch the fifth intercostal space so as to receive the blow of the apex

of the heart. The liquid in the tube is now observed to be in motion. With persons in ordinary health, the liquid rises and falls about an inch. This rise and fall, after taking place three or four times, is followed by a much longer rise and fall to the extent of three or four inches, due to the advancement and retirement of the wall of the chest during the acts of respiration. The shorter rise and fall are again repeated and are again followed by the longer rise and fall caused by the motions of the chest. During the longer rise and fall due to respiration, the beat and retreat of the heart are still to be recognized by brief interruptions in the rise and fall of the liquid. When difficulty is experienced in obtaining the shock of the heart sufficiently strong to give an appreciable rise and fall, the examinee should make a moderate expiration, and then hold his breath and incline the chest somewhat forward. When the action of the heart is feebly felt at the præcordial region, it may be necessary to apply the instrument to the naked chest; but this is not necessary in the great majority of cases, and it will generally suffice to make the shirt and waistcoat fit tight to the skin. In many trials the sphygmoscope has succeeded in indicating the movements of the heart through the tightly buttoned coat. Thin persons are very favourable for examination; on the other hand, the corpulent less readily affect the instrument.

The movements of the heart, though best indicated at the fifth intercostal space, are to be denoted at other parts of the chest, and in some examples of disease and of large and powerful heart, even in the epigastric region. The moving arm proves convenient in applying the instrument to these parts. In many persons with no very excited heart, it is sensibly acted on at the scapular and infra-dorsal regions.

By means of this instrument the observer can ascertain the frequency of the beats of the heart, but as this can be effected in most cases with accuracy at the radial artery, no particular advantage is gained from it in respect to this point.

The duration of the impulse of the heart upon the chest is well measured by this instrument: the time occupied by the rise, is the time occupied by the impulse. A slow rise after a rapid rise shows a slow beat after a rapid one, and *vice versâ,* a slow fall after an ordinary fall, shows a slow retirement after an ordinary one. An

intermittent pulse is marked by an imperfect rise, followed by an ordinary fall, and then by a long rise. The rise is sometimes slower than the fall, which is occasionally found to be abrupt. This is observed when the heart, by reason of its great size, and of the somewhat bent back posture of the thorax, suddenly falls away from the walls of the chest.

The movements of the auricles under ordinary circumstances are not indicated by the sphygmoscope, though when it is placed over them, the liquid in the tube is moved upwards and downwards; but as these movements are synchronous with the movements upwards and downwards of another instrument placed at the apex, it may be inferred that the ventricle is the cause of them all. In some examples of greatly excited heart, as in phthisis, the instrument has revealed movements which seemed to proceed from the auricles; but further observations are required to settle this point, as well as the question whether the movements of the aorta, in a state of excitement, communicate any influence to the instrument.

The instrument, placed upon the heart, indicates strokes of that organ which are so feeble as to have no corresponding pulse at the wrist.

No pause whatever in the movement of the liquid has been at any time observed when the sphygmoscope has been carefully placed so as to receive the full beat, and fall back with freedom. This would go to show that the heart, however slow, is in constant motion, and, contrary to the belief of many physiologists, enjoys no pause. There is certainly no pause in the descent of the liquid, which takes place when the heart retires from the thoracic walls, in the middle of which movement it has been said a very short pause is to be observed in living animals having the heart exposed.

The force with which the heart beats at the fifth intercostal space may be ascertained by closing the upper extremity of the glass tube, and observing the extent to which the enclosed air is compressed.

When the heart is excited, the liquid in the sphygmoscope rises and falls more than usual; but the rise and fall of the excited enlarged heart is much the same as the rise and fall of the excited normal organ. For the most part the enlarged heart gives movements to the instrument when placed upon the ribs and sternum, whilst the

normally sized heart affects more exclusively when it is placed upon the fifth intercostal space.

The sphygmoscope indicates with exactitude both the absolute and the comparative influence upon the heart, of food, cordials, stimulants, and tonic medicines. It does the same in respect to depressing causes, such as hunger, cold, and sedatives.

With the aid of this instrument the fact is demonstrated, that the action of the heart may be great when the pulse is small,—that the heart may strike the instrument with force when the pulse scarcely affects the liquid of the hand-sphygmoscope. It affords a remarkable proof that the pulse is one thing and the heart's action another, and teaches that the pulse is only an approximate sign of the state of the heart. It is found also, that while cold at the surface and extremities may depress the pulse, the heart may remain little enfeebled, or even become excited, and that warmth and friction applied to the extremities may cause an excited pulse without there being any accompanying increased force of the heart.

The influence of respiration upon the action of the heart is manifested, in some degree, by the instrument placed over the region of the heart. If the breath be stopped after an ordinary expiration, the movement of the liquid is seen to be increased. If a very long and forcible inspiration be made and the breath then suspended, the movement is somewhat reduced; but when the respiration is again allowed to take its normal course, the movement is seen to be increased for a short time.

The sphygmoscope rises during the first sound of the heart and falls at the second.

The sphygmoscope reduced (fig. 2), deprived of its stand, having a level elastic wall instead of protruding one, and having a glass tube with an almost capillary bore, forms a remarkably delicate indicator of the pulse*. It is so delicate in its impressions that it

* Since this instrument was contrived, the author has learned that a sphygmometer of much the same construction was invented some twenty years ago by Mons. le Docteur Hérrison, and that a memoir upon it was presented to the Institute of France. The liquid employed was mercury—too heavy to indicate feeble impulses, and the moveable wall was of gold-beater's skin, which is inelastic. It may be added, that M. Magendie reported against the practical application of the invention.

is appreciably affected by the regurgitant wave in the jugular veins, and by the wave in arteries greatly smaller than the radial. From its nicety in manifesting the beat of the blood-wave, it is very valuable, and is called the hand-sphygmoscope.

Fig. 2.

Hand-sphygmoscope.

By means of this hand instrument applied to the arteries, a comparison is readily made between the time of the beat of the heart and the rise of the arteries under the influence of the blood-wave. This instrument is much more delicate than the finger in such an inquiry. The impressions made upon the fingers of two hands fail to be conveyed with sufficient nicety to the mind to tell with certainty the relative time of the beat of the heart and arteries. Except in cases of extreme slowness, the sensations obtained from the two hands impressed at nearly the same time, do not admit of a distinct difference in respect to time being made out. It has been to this very defect that the erroneous idea, that the beat of the heart and the beat of the pulse are synchronous, or nearly so, has owed its origin and continuance.

The hand-sphygmoscope, placed upon the radial artery, shows a rise of the liquid while there is a fall in the sphygmoscope placed over the heart. As the liquid in the one instrument starts from below, the liquid in the other starts from above, and as the liquid in the one reaches the top of its ascent, the liquid in the other reaches the bottom of its descent, to renew their opposing course. The movements in the two instruments at the same instant are always opposed, and the whole time occupied in the movement of one instrument in one direction appears to be occupied by the movement of the other in the opposite direction. The movements *alternate* with as much apparent exactitude as the arms of a well-adjusted balance. When the lapse of time between the beat of the heart and the pulse at the wrist was first observed, suspicion of disease of the aorta was entertained, but the subsequent examination of many persons proved that this alternation was natural. In some twenty persons subjected to examination, the complete alternation has been made out without the shadow of a doubt. These persons were of all ages above childhood, and had the pulse of different degrees of rapidity from 60 to 100.

Hand-sphygmoscopes placed upon the carotid, the brachial, the radial, the femoral, and the dorsal artery of the foot, rise at the same instant, and fall at the same point of time.

These facts prove the existence of two great laws not previously enunciated,—1st, that the heart's beat alternates with the pulse at the wrist; 2ndly, that the pulse of arteries beyond the chest takes place in all parts at the same instant, and without any appreciable interval.

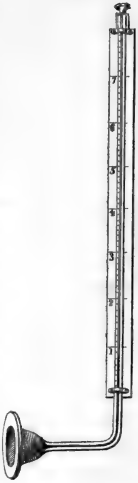
The pulse, it appears, occurs during the retirement of the heart from the thoracic walls, and the collapse or fall of the arteries takes place during the impulse of the heart. During the rise in the hand-sphygmoscope placed over the arteries, the second sound of the heart has been distinctly heard, and during the fall, the first, softer and more prolonged sound has been easily distinguished.

The horse has been subjected to examination, to learn the relative time of the beat of the heart and arteries, but the respiratory movements and the motions of the animal have hitherto restricted the application of the instruments. However, it has been most distinctly ascertained, by the hand placed upon the heart and upon the plantar artery, that between the beat of these parts there is a decided interval. The slowness of the action of the heart in the horse renders this experiment less open to error than in man. In these experiments upon the horse, Mr. Mavor, the eminent veterinarian, gave his valuable aid.

The sphygmoscope forms a good pneumoscope. It delicately measures the rise and fall of the chest in respiration. It likewise declares the relative duration of inspiration and expiration, and may thus prove useful in the detection of incipient phthisis, and other pulmonary diseases. When the liquid has attained its highest elevation at the end of inspiration, it immediately begins to fall; but when it has reached the lowest point at the end of expiration, it remains there some instants. The ascent is slower than the descent. After the fall of an ordinary expiration, a forced expiration gives a second fall.

The sphygmoscope may be employed without a stand and is then more portable (fig. 3), but from the want of a fixed basis, and from the motion of the ribs on which it must rest, its manifestations are less extensive and satisfactory. It may be maintained *in situ* with an elastic band placed around the thorax. When em-

Fig. 3.



Portable Sphygmoscope.

ployed without a stand, as it must rest upon the ribs, the elastic wall of the chamber should be plain, and not protruding.

The hand-sphygmoscope is an exceedingly delicate instrument, but requires great care and nicety in its construction. It may be made by taking about an inch and a half of a gutta percha tube, half an inch in diameter, slightly widening one extremity of it to make a chamber large enough to hold a small horse-bean, and fastening with thread a piece of thin india-rubber, or of Bourgeaud's india-rubber bandage, securely over it for the elastic and moveable wall. The liquid is now supplied, and the glass tube, with a very fine flat bore, say $\frac{1}{10}$th of an inch, and provided with a ring of india-rubber, obtained by cutting off a small portion of a fine india-rubber tube, for a "washer," is now inserted and the instrument is ready for use. The hand-sphygmoscope discovers the blood-wave in regurgitation of the jugular veins; it responds to the radial of the newly-born infant; it rises and falls with the movements of the brain of the infant, though some months old, as that organ rises and falls under the influence of its arteries. There is no doubt that, applied to the fontanelles before delivery, it will inform the obstetrician whether the fœtus be dead or alive, and, in cases of difficult labour, supply important evidence for his guidance.

The hand-sphygmoscope applied to the radial artery, and to the fontanelles of a dying infant three months old, has indicated to the author the influence of respiration upon the circulation. During inspiration, the column of liquid in the tube was found to fall as if sucked down, and during expiration to spring again.

In practical surgery, the hand-sphygmoscope may possibly be employed with advantage, for it will rise with the wave or fluctuation of liquid tumours. It may be placed where the fingers cannot reach. The rise in the instrument is greater in liquid than in aëriform tumours on account of the compressibility of air, and the fall is more rapid and decided when the contents of the tumour are liquid.

For the most part, the hand-sphygmoscope is best applied simply with the aid of the fingers. It is delicately held between the tips of the thumb, fore- and middle-fingers, the nails resting on the examinee. The elastic wall is on no account to be pressed down with a dead weight upon the vessel. It is to be nicely lowered to the level of the artery when collapsed. When the artery rises, it will strike the elastic wall, and as the chamber is fixed by the fingers, the entire blow is communicated to the liquid and it rises in the tube. During the retirement or collapse of the artery, the elastic wall resumes its level condition and draws the liquid down the tube. This motion of the liquid allows the instrument to be employed though the open end of the tube be dependent. When it is desired to avoid the varying pressure experienced when the instrument is held between the fingers, some such apparatus as was invented by Dr. E. S. Blundell, or an elastic band suitably applied around the wrist, will be useful.

The sphygmoscope is for several purposes rendered more convenient of application by interposing, between the chamber and the glass tube, a piece of india-rubber tube of suitable bore and length. In this way the comparison of the beat of the heart and the pulse of an artery is much facilitated, for the glass tubes of the two instruments employed may be brought parallel and close to each other, so that the opposite motions of the liquids in the two tubes are, by near contrast, rendered easier of observation. In employing this adaptation, care must of course be taken that the india-rubber tube is of the same calibre and length in both instruments.

It is hoped that the sphygmoscope will aid in the acquisition of additional knowledge of the movements and condition of the heart, the situation of which within a case of bone, wisely provided to secure it from injury, has this disadvantage for the physiologist and physician, that the action and condition of the organ are with difficulty made out. By means of the sphygmoscope, that small amount of movement which is manifested at the exterior of the chest may be rendered more appreciable to our senses, and more available for physiological and curative purposes; and perhaps information may be obtained by this instrument which has hitherto been procurable only by the practice of vivisection.

Park Street, Grosvenor Square, London,
Jan. 12, 1856.

February 7, 1856.

Colonel SABINE, R.A., V.P. and Treasurer, in the Chair.

The following communications were read :—

I. "On the Vitality of the Ova of the Salmonidæ of different Ages; in a Letter addressed to CHARLES DARWIN, Esq., M.A., V.P.R.S. &c." By JOHN DAVY, M.D., F.R.S. Lond. and Edinb. &c. Received January 15, 1856.

MY DEAR SIR,—In a letter which I had the honour to address to you last year "On the Ova of the Salmon in relation to the distribution of Species," I have expressed the hope that some of the results of observations therein described may aid in solving the question as to the period, the age, at which the impregnated ova of fish are most retentive of life, and consequently are in the state best fitted for transport without loss of life.

Joining with you in considering the subject in need of and deserving further inquiry, I have taken the earliest opportunity that has offered of resuming it. The experiments which I have made, and which I shall now describe, have been more limited than I could have wished, having been confined to the ova of the Charr, as I was not able to obtain the ova of the Salmon or any of its congeners in a fit state for the trials required.

The ova of the Charr which have been the subject of my experiments, were from living fish brought to me from the river Brathay, a tributary of Windermere, on the 9th of November. They were obtained by the pressure of the hand on the abdomen of the females under water, and immediately after their expulsion a portion of liquid milt, procured in the same way from a male, was mixed with them for the purpose of impregnation.

The ova thus treated, 654 in number, procured from two fish,

were transferred, after little more than an hour, to a shallow glazed earthenware pan, of a circular form, about a foot in diameter, without gravel, the water in which, afterwards, was changed daily once, and once only. The vessel was kept in a room of a temperature fluctuating from about 55° Fahr. when highest, to about 40° when lowest. The water used was well-water of considerable purity, and before used it was allowed to acquire the temperature of the room.

Two modes occurred to me as likely to afford the means of testing the vital power of the ova, or their power of endurance without loss of vitality; viz. one by subjecting them for a limited time to a temperature raised above the ordinary temperature; the other, by having them conveyed to a considerable distance.

For the trials first proposed, the ova were put into a thin glass vessel half-full of water, which was placed in a water-bath and heated to the temperature desired.

The first experiment was made on ova taken from the general stock one day after their expulsion. Six, for two hours, were exposed to a temperature varying from 79° to 80° of Fahr. The result was, that they became opaque in the course of twenty-four hours, all but one, and that, some days after, underwent the same change, denoting loss of vitality.

The second experiment was made on the 10th of November. Six ova were similarly exposed for two hours to a temperature rising gradually from 70° to 78°; the result was similar: on the following day they were all found opaque.

The third experiment was made on the 11th of November. The same number of eggs were exposed for an hour to a temperature falling from 70° to 69°. Two shortly became opaque; four retained their transparency during a month, though in reality dead, which was denoted by their bearing no marks of development when seen under the microscope, those ova which retained their vitality being at that time well advanced.

The fourth experiment was made on the 1st of December; the ova, the same number, were exposed to a temperature rising from 75° to 78° for an hour and twenty-two minutes. Three became opaque, other three retained their transparency and vitality, and in due time were hatched, the first on the 31st of December, the last on the 7th of January.

The fifth experiment was made on the 13th of December. Six ova were exposed for an hour and twenty-five minutes to a temperature falling from 82°, which it was at the beginning, to 78°, which it was at the end. Two became opaque; in these no marks of progress could be seen of development, thus indicating that they were dead at the time of trial. Four remained transparent; in these, under the microscope, embryo-fish were seen with an active circulation of the blood-corpuscles. One of them was hatched on the 31st of December; one, the last, on the 6th of January.

The sixth experiment was made on the 20th of December, on six ova, containing living embryos. They were exposed for an hour and twenty-eight minutes to a temperature of about 98°, and this during the whole time. When taken out, they had not lost their transparency, but in each the heart's action was arrested, and death was the result: they all sooner or later became opaque, from the common cause, the imbibition of water.

The seventh experiment was made on the 21st of December, on six ova, in which the circulation was distinct in the fœtal fish. After an exposure for an hour and five minutes to a temperature of 70° rising to 82°, in five, on cooling, the circulation was found active; in one, stopped, which was dead; two were hatched on the 5th of January; three, the remainder, on the 7th of the same month.

The eighth experiment was made on the 23rd of December, on six ova, each containing a living fœtus. They were exposed to a temperature falling from 84° to 82° during an hour and twenty minutes. Examined after the water had cooled, in one, the circulation was seen pretty distinct; in two, very feeble; in three, the blood-corpuscles appeared to be stagnant. Examined on the following day, the circulation was seen active in all. One was hatched on the 5th of January, the other five in the two following days.

The ninth experiment was made on the 24th of December. Six ova were exposed for two hours and four minutes to a temperature falling from 72° to 70°. Examined a quarter of an hour after, and before the water was cold, the circulation was found vigorous in all. One was hatched on the 2nd of January, the remainder between the 5th and 8th.

The tenth experiment, and the last of its kind that I have to describe, was made on the 2nd of January. Six ova, in each of which

the circulation was distinct, were exposed for four hours to a temperature varying from 70° to 72°—the greater part of the time 72°. Examined immediately on being taken out, the circulation was seen uninterrupted in three, arrested in the other three. In three-quarters of an hour, when the water had cooled nearly to the temperature of the room, 55°, the circulation was found to be renewed in the latter. In the interval, one of the former was hatched, and a vigorous fish produced; on the following morning four more had come forth, and in the one remaining egg the fœtal circulation was vigorous; it was hatched on the 4th of January.

I beg now to pass to the other series of experiments referred to, those in which trial of the vitality of the ova was made by sending them to a distance. The method was briefly the following. The ova were lightly packed in wet wool contained in a tin-plate box perforated in its bottom to admit air, and covered with a wooden cover that had been soaked in water, with the intent of preserving moisture. The box was wrapped in tow, loosely covered with oiled paper, and the whole, in an envelope of common writing-paper, was well secured by a binding of thread. Thus prepared, the ova were sent by post to Penzance, in Cornwall, a distance exceeding 500 miles, with the request that they should be sent back by return of post unopened.

The first experiment was made on the 9th of November. The number of ova sent was thirty, taken from the common stock without selection. They were received on their return on the 14th of the same month. On taking them out, all were found transparent; but, with the exception of one, all became opaque on being put into water, and that one, after a few days, also underwent the same change.

The second experiment was made on the 14th of November. Twenty ova then sent were returned on the 18th. All became opaque on being put into water.

The third experiment was made on the 1st of December. Twenty ova then sent were returned on the 5th. Put into water, eleven became opaque within a minute; most of these were slightly shrivelled. After three hours, two more became opaque. After forty-eight hours, four only remained transparent; in these, under the microscope, the circulation was found active in two; in the other two it could not be detected. One was hatched on the 31st of December, the other died before hatching.

The fourth experiment was made on the 13th of December. Twenty-two ova then sent came back on the 17th. During the interval there was a severe frost; the thermometer here in the open air was constantly below the freezing-point, and it would appear to have been much the same throughout England. When examined, eleven of the ova immediately became opaque on immersion in water. In the other eleven there was no loss of transparency, and in these, under the microscope, the circulation was found active. Those which had become opaque were placed in a pretty strong solution of common salt, by which their transparency was restored, the saline solution dissolving the coagulum. Now examined, no traces of development could be detected under the microscope in any one of them,—showing that they had been dead before they were sent away.

On the following day, the 18th of December, the eleven transparent ova were repacked, and again sent the same distance. They came back on the 22nd; they retained then their transparency; placed in water, a feeble circulation was to be seen in two under the microscope; in nine the blood-corpuscles had ceased to flow; these became opaque. Of the two in which the circulation was perceptible, one was hatched on the 28th of December; the young fish in the other died, it would appear, in the act of breaking the membrane, its head, on the 29th, having been found protruding, but the heart's action stopped.

The fifth experiment was made on the 26th of December. Ten ova, in which the circulation was active, and the fœtus in each well advanced, were sent off on the day mentioned, and returned on the 31st. The weather, during the whole time, was mild, the frost having ceased. When opened, the ova were all found hatched, and the young fish dead, as might have been expected. When put into water, not one of them showed any signs of remaining vitality; they were all examined under the microscope.

The sixth and last experiment was made on the 6th of January. Six ova, in each of which the circulation was vigorous, were put into a glass tube of one cubic inch and a half capacity, with water to the height of about 1·4 cubic inch, the remaining space, after closure by a cork, being filled with air. The intention was to try the effects of conveyance to a distance on these ova in water with a small quantity

of air. Owing to a mistake, they were not forwarded. Examined on the following day, five ova were found hatched, the young fish dead; in the one ovum remaining unhatched, the fœtus was alive, the circulation active; on the 9th it burst its shell; the young fish was vigorous.

As I could not with any certainty determine, at the time the experiments were commenced, what eggs were impregnated and alive, and what were not, I had at the beginning thirty ova taken indiscriminately from the common stock, and put apart in a glass vessel, the water in which was also changed daily. Of this number, seven were found in progress of development on the 14th of December, or 23 per cent.; the rest had become opaque. One of the seven was hatched on the 31st of December, the others in succession, the last on the 8th of January.

Further to arrive at a proximate average of the proportion of impregnated and unimpregnated ova, or living and dead, on the 14th of December, when in the living ova the circulation was distinct under the microscope, and the embryos were visible even to the unaided eye, I examined the whole number then remaining, viz. 405, thus reduced, owing to 67 having been removed, one after another having become opaque, and 152 having been taken out for the purpose of experiments. Of these 405 remaining, 138 were found alive, each containing a well-formed embryo, and 267, though still transparent, without life, no marks of organization being to be seen in them, either with the naked eye or under the microscope. Hence, irrespective of the 152 experimented on, the proportion of living to dead on the 14th of December would appear to be as 138 to 364, or about 25 per cent. And, with the exception of two which died after the 14th, all those then alive were hatched, the first on the 31st of the same month, the last on the 9th of January.

What are the conclusions to be drawn from these results? From those of the first series of experiments, may it not be considered as proved that the power of resisting an undue increase of temperature is possessed in a higher degree by the ova in an advanced than in an early stage of development,—the degree probably being in the ratio of the age? From those of the second series, is it not as manifest that the power of bearing distant transport, and of retaining life in moist air, is in like degree increasing with age? And from both,

may not the general conclusion be drawn, that the strength of vitality of the impregnated ovum, or its power of resisting agencies unfavourable to its life, gradually increases with age and the progress of fœtal development? And as the Charr is one of the most delicate of the family of fishes to which it belongs, may it not further be inferred, with tolerable confidence, that the ova of the other and more hardy species of the Salmonidæ, were they similarly experimented upon, would afford like results, confirmatory of those obtained last year in some trials on the ova of the Salmon, and mentioned in my former letter to you?

The practical application of these results, and of the conclusions deducible from them, is obvious, and need not at present be dwelt upon.

I am, my dear Sir,
Yours very truly,
JOHN DAVY.

Lesketh How, Ambleside,
January 10, 1856.

II. "Note on a new Class of Alcohols." By M. AUG. CAHOURS and A. W. HOFMANN, Ph.D., F.R.S. &c. Communicated by Dr. HOFMANN. Received January 31, 1856.

On submitting to dry distillation glycerine, either alone or together with bisulphate of potassium or anhydrous phosphoric acid, M. Redtenbacher obtained a remarkable product, to which he gave the name of acroleine. Presenting all the characters of an aldehyde, and approximating more particularly to vinic aldehyde by the general aspect of its reactions, this substance changes under the influence of oxidizing bodies, especially of oxide of silver, an acid being formed, named by this philosopher acrylic acid, an acid which stands in the same relation to acroleine as acetic acid does to aldehyde.

The researches of MM. Will and Wertheim on the essential oils of mustard and of garlic, tended to indicate a relation between these substances on the one hand and acroleine and acrylic acid on the other, a result which was established by the more recent investiga-

tions of MM. Berthelot and De Luca. On studying the action of iodide of phosphorus on glycerine, these chemists obtained an iodine-compound named by them iodide of propylene, which is an analogue of the chloride and bromide of propylene, previously produced by MM. Cahours, Reynolds, and Hofmann, when submitting to the action of chlorine and bromine the gases which are formed when either amylic alcohol or valeric acid and its homologues are exposed to the influence of heat.

MM. Berthelot and De Luca have further shown that the result of the mutual decomposition of iodide of propylene and sulpho-cyanide of potassium is an oil identical with that obtained on distilling the seeds of black mustard with water in an alembic. By this remarkable experiment it is most clearly demonstrated that the volatile oil of mustard belongs to the propylene series, a relation which had been previously pointed out by Capt. Reynolds, but which he has omitted to establish by experiment. If, then, we admit the existence of a hydrocarbon, C_6H_5, analogous to ethyl, C_4H_5, we get

C_6H_5Cl	Chloride of propylene.	C_4H_5Cl	Chloride of ethyl.
C_6H_5Br	Bromide of propylene.	C_4H_5Br	Bromide of ethyl.
C_6H_5I	Iodide of propylene.	C_4H_5I	Iodide of ethyl.
C_6H_5S	Essential oil of garlic.	C_4H_5S	Sulphide of ethyl.
$C_6H_5C_2NS_2$	Essential oil of mustard.	$C_4H_5C_2NS_2$	Sulphocyanide of ethyl.
$C_6H_4O_2$	Acroleine	$C_4H_4O_2$	Aldehyde.
$C_6H_4O_4$	Acrylic acid.	$C_4H_4O_4$	Acetic acid.

All that now remained was to discover the keystone of this edifice, in other words, to establish the existence of an alcohol to which the preceding compounds might be referred, and by the aid of which a still more numerous series of ethers, both simple and compound, and analogous in every respect to the derivatives of ordinary alcohol, might be obtained. After many protracted and unsuccessful attempts, we have succeeded in producing the alcohol and ether of this series, for which we propose retaining the name of the acryl series.

In order to arrive at this result, we have submitted several silver-salts to the action of iodide of acryl. There are but few acids whose salts lend themselves conveniently to this reaction. Among the various salts which we have examined with this view, the oxalate of silver has furnished the most satisfactory results. This salt is most violently attacked by iodide of acryl; the reaction is complete after

two or three hours' digestion. The *oxalate of acryl* formed in this process, when separated from the iodide of silver, washed with water, dried over chloride of calcium, and redistilled, presents itself as a colourless transparent liquid, heavier than water, possessing a peculiar aromatic odour. It boils at 207°, and by analysis has been proved to contain

$$C_8 H_5 O_4 = C_2 (C_6 H_5) O_4.$$

When treated with ammonia, oxalate of acryl furnishes oxamide and the alcohol, which was the object of our researches. This alcohol—*acrylic alcohol*—is a colourless transparent liquid of a peculiar, somewhat pungent odour, resembling that of mustard, and which in fact is more or less characteristic of nearly all the members of the acryl series.

The analysis of acrylic alcohol has led to the formula

$$C_6 H_6 O_2 = 4 \text{ vol. of vapour.}$$

This compound is isomeric with acetone and with propyl-aldehyde, from which substances, however, it differs essentially by the aggregate of its properties.

Acrylic alcohol burns with a much more luminous flame than ordinary alcohol. It mixes in all proportions with water. Treated with potassium, it disengages hydrogen and is converted into a transparent gelatinous mass, which is the *acryl-term corresponding to potassium-alcohol.*

This potassium compound is violently attacked by iodide of acryl; a precipitate of iodide of potassium is thrown down, and a liquid is formed lighter than water, and insoluble in this fluid. This new substance corresponds to ordinary ether; its formation is illustrated by the following equation:—

$$C_6 (H_5 K) O_2 + C_6 H_5 I = KI + C_{12} H_{10} O_2.$$

The same product is formed by the action of iodide of acryl upon oxide of silver or of mercury.

On treating the new potassium-alcohol with iodide of ethyl, or the ethyl-potassium-alcohol with iodide of acryl, an aromatic liquid is produced, which is obviously the *mixed ether of the ethyl and acryl series.*

If acrylic alcohol be distilled with chloride, bromide or iodide of

phosphorus, the *chloride, bromide* and *iodide of the acryl series* are reproduced with the greatest facility.

Acrylic alcohol dissolves in concentrated sulphuric acid, without separation of carbon; the liquid, mixed with water and neutralized with carbonate of barium, furnishes a crystalline salt, which contains,

$$C_6 H_5 BaS_2 O_8 = BaSO_4, (C_6 H_5) SO_4.$$

This is the *sulphovinate of the series.*

On treating the mixture of acrylic alcohol with concentrated sulphuric acid, a most violent reaction takes place; the alcohol is entirely carbonized with evolution of sulphurous acid.

Anhydrous phosphoric acid affects the alcohol with less energy. The mass darkens with evolution of a transparent colourless gas, burning with a luminous flame. The analysis of this gas remains to be made.

Acrylic alcohol is rapidly attacked by oxidizing agents. A mixture of sulphuric acid and bichromate of potassium acts with tremendous violence; the products of the reaction being acroleine and acrylic acid, or its products of decomposition. The same transformation is effected by spongy platinum.

When treated with potassa and bisulphide of carbon, the new alcohol solidifies at once into a mass of splendid yellow needles, which correspond to xanthate of potassium.

By the aid of the alcohol itself, its sulphovinic acid, or its iodide, all the terms of the acryl series may be produced with the greatest facility. We will specify the following compounds, the study of which we have more or less completed.

Acryl-oxamethane, or *oxamate of acryl,* is readily formed by adding alcoholic ammonia in small quantities to oxalate of acryl, until a permanent precipitate is produced. The filtered solution deposits on evaporation the oxamate in magnificent crystals.

Carbonate of acryl is an aromatic oily liquid, lighter than water. It is formed like the other carbonic ethers, by the action of sodium upon the oxalate. An alcoholic solution of this substance, when treated with baryta, furnished carbonate of barium and acrylic alcohol.

Benzoate of acryl is readily produced by the action of chloride of benzoyl upon acrylic alcohol. It is a liquid heavier than water,

which boils at 220°, and possesses an aromatic odour, similar to that of benzoic ether. The analysis of this substance leads to the formula

$$C_{20}H_{10}O_4=C_{14}[H_5(C_6H_5)]O_4.$$

The same body is easily produced by the mutual reaction of iodide of acryl and benzoate of silver.

Acetate of acryl, obtained by the action of iodide of acryl upon acetate of silver, is a liquid lighter than water, of an odour resembling that of common acetic ether. According to our analysis, it contains

$$C_{10}H_8O_4=C_4H_3(C_6H_5)O_4.$$

Cyanate of silver is most violently attacked by iodide of acryl, even in the cold. The heat generated during this reaction is so powerful that the whole of the new product distils over. The substance thus obtained has an incredibly penetrating odour, and causes lacrymation in the highest degree. The analysis of this colourless transparent liquid, which boils at 82°, led to the formula

$$C_8H_5NO_2=C_2(C_6H_5)NO_2.$$

This is the *cyanate of acryl*. Gently warmed with a solution of ammonia, this liquid readily dissolves, and the solution deposits upon evaporation magnificent crystals, which are nothing but acrylic urea

$$C_8H_8N_2O_2=C_2H_3(C_6H_5)N_2O_2$$

corresponding to thiosinnamine, the long-known sulphur-urea term of this series,

$$C_8H_8N_2S_2=C_2(H_3, C_6H_5)N_2S_2.$$

Aniline produces with cyanate of acryl an analogous substance, which crystallizes remarkably well.

When treated with water, cyanate of acryl is gradually converted into a solid crystalline substance. The compound obtained in this manner has the composition and all the properties of *sinapoline* or *diacrylic urea*. Its formula is

$$C_{14}H_{12}N_2O_2=C_2[H_2(C_6H_5)_2]N_2O_2,$$

and its formation is illustrated by the following equation:—

$$\underbrace{2C_8H_5NO_2}_{\text{Cyanate of acryl.}}+2HO=\underbrace{C_{14}H_{12}N_2O_2}_{\text{Sinapoline.}}+2CO_2.$$

Cyanate of acryl is decomposed by a concentrated solution of

potassa; a solid substance is rapidly formed, which floats upon the surface of the solution, and which is nothing but the same sinapoline, whilst a strongly alkaline liquid passes into the receiver, which is a mixture of several bases, in which we have traced already

(1) Methylamine.
(2) Propylamine.
(3) Acrylamine.

The latter substance boils between 180° and 190°. All our attempts to produce a well-crystallized platinum-salt of this base have hitherto failed.

The experiments detailed in the preceding sketch incontestably demonstrate the existence of a new series of alcohols, the third term of which is acrylic alcohol.

Like ordinary alcohol, this new alcohol furnishes a series of derivatives, which may be formulated in a similar manner.

The following tables exhibit the terms of the acryl series hitherto prepared in juxtaposition with the corresponding members of the ethyl series:—

Acryl Terms.		Ethyl Terms.
$C_6\ H_6\ O_2$	Alcohol	$C_4H_6\ O_2$
$C_6\ H_5\ O$ or $C_{12}H_{10}O_2$	Ether	$C_4H_5\ O$ or $C_8H_{10}O$
$C_6\ H_5\ Cl$	Chloride	$C_4H_5\ Cl$
$C_6\ H_5\ Br$	Bromide	$C_4H_5\ Br$
$C_6\ H_5\ I$	Iodide	$C_4H_5\ I$
$C_6\ H_5\ S$ or $C_{12}H_{10}S_2$	Sulphide	$C_4\ H_5\ S$ or $C_8\ H_{10}S_2$
$C_2(K, C_6H_5)S_4O_2$	Xanthate of potassium	$C_2(K, C_4H_5)\ S_4\ O_2$
$C_2\ (C_6H_5)\ NS_2$	Sulphocyanide	$C_2(C_4H_5)\ NS_2$
$C_2(C_6H_5)\ NO_2$	Oxycyanide or cyanate	$C_2(C_4H_5)\ NO_2$
$C_2(H_3C_6H_5)N_2S_2$	Sulphuretted acryl-urea—Thiosinnamine	?
$C_2(H_3, C_6H_5)N_2O_2$	Acryl-urea, Ethyl-urea	$C_2\ (H_3, C_4H_5)\ N_2O$
$C_2[H_2(C_6H_5)_2]N_2O_2$	Diacryl-urea, Diethyl-sinapoline-urea......	$C_2\ [H_2\ (C_6H_5)_2]\ N_2O_2$
$C_2\ (C_6H_5)\ O_4$ or $C_4\ (C_6\ H_5)_2\ O_8$	Oxalate	$C_2\ (C_4H_5)\ O_4$ or $C_4(C_4H_5)_2\ O_8$
$C_4H_2(C_6H_5)\ O_4$	Oxamate	$C_4\ (H_2, C_4H_5)\ O_6$

Acryl Terms.		Ethyl Terms.
$C\,(C_6H_5)\,O_3$ or $C_2\,(C_6H_5)_2\,O_6$	Carbonate	$C\,(C_4H_5)\,O_3$ or $C_2\,(C_4H_5)_2\,O_6$
$C_4H_3\,(C_6H_5)\,O_4$	Acetate	$C_4\,(H_3, C_4H_5)\,O_4$
$C_{14}H_5\,(C_6H_5)\,O_4$	Benzoate	$C_{14}H_5\,(C_4H_5)\,O_4$
$C_6H_5\,SO_4\,HSO_4$	Sulphovinic acid ..	$C_4H_5\,SO_4\,HSO_4$
$(C_6H_5)\,H_2N$	Acrylamine, Ethyl-amine	$(C_4H_5)\,H_2N$
$C_6H_4O_2$	Aldehyde. Acrylic, Ethylic, Acroleine	$C_4H_4O_2$
$C_6H_4O_4$	Acid. Acrylic; acetic	$C_4H_4O_4$
C_6H_6	Hydrocarbon. Propylene ?, Acetene	C_4H_6

Acrylic alcohol, the history of which we have endeavoured to sketch in the preceding pages, and in the study of which we are now engaged, is the third term of a series of alcohols, which is parallel to the ordinary alcohols of the formula

$$C_{n2}\,H_{n2+2}\,O_2,$$

and the prototype of which is ethylic alcohol. The acid corresponding to this alcohol is acrylic acid, as has been stated. Chemists are already acquainted with several homologues of acrylic acid, which stand to the series of fatty acids in the same relation which exists between our new alcohol and common alcohol. Cyanide of acryl, which is readily procured by the action of iodide of acryl upon cyanide of silver, but which as yet we have not been able to obtain in a state of perfect purity, when submitted to the action of potassa, will obviously furnish an acid, homologous to acrylic acid equally as cyanide of propyl is transformed into butylic acid.

We terminate this note with a synoptical table of the two homologous groups.

Group of Alcohols.				Group of Acids.			
$C_2H_2O_2$		$C_2H_4O_2$	Methylic	C_2O_4	(Carbonic ?)	$C_2H_2O_4$	Formic
$C_4H_4O_2$		$C_4H_6O_2$	Ethylic	$C_4H_2O_4$		$C_4H_4O_4$	Acetic
$C_6H_6O_2$	Acrylic	$C_6H_8O_2$	Propylic	$C_6H_4O_4$	Acrylic	$C_6H_6O_4$	Propionic
$C_8H_8O_2$		$C_8H_{10}O_2$	Butylic	$C_8H_6O_4$		$C_8H_8O_4$	Butylic
$C_{10}H_{10}O_2$		$C_{10}H_{12}O_2$	Amylic	$C_{10}H_8O_4$	Angelic	$C_{10}H_{10}O_4$	Valeric
$C_{12}H_{12}O_2$		$C_{12}H_{14}O_2$	Caproic	$C_{12}H_{10}O_4$		$C_{12}H_{12}O_4$	Caproic
$C_{14}H_{14}O_2$		$C_{14}H_{16}O_2$		$C_{14}H_{12}O_4$		$C_{14}H_{14}O_4$	Œnanthylic
$C_{16}H_{16}O_2$		$C_{16}H_{18}O_2$	Caprylic	$C_{16}H_{14}O_4$		$C_{16}H_{16}O_4$	Caprylic
——		——		——		——	
——		——		——		——	
$C_{36}H_{36}O_2$		$C_{36}H_{38}O_2$		$C_{36}H_{34}O_4$	Oleic	$C_{36}H_{36}O_4$	Stearic

This table exhibits a considerable number of gaps, which the progress of science will not be long in filling up. Even now we have established by experiment that bromide of amylene suffers many changes, which are perfectly analogous to those which we have witnessed in the acryl series, and even the derivatives of olefiant gas appear to exhibit in many respects an analogous deportment.

February 14, 1856.

Dr. W. A. MILLER, V.P., in the Chair.

The following communication was read :—

"On Periodical Laws discoverable in the Mean Effects of the larger Magnetic Disturbances."—No. III. By Colonel EDWARD SABINE, R.A., D.C.L., Treas. and V.P.R.S.

(Abstract.)

In two previous papers bearing the same title as the present (Phil. Trans. 1851, Art. V., and 1852, Art. VIII.), the author showed, from the hourly observations of the magnetic Declination at Toronto and Hobarton, that the magnetic disturbances of large amount, and apparently irregular occurrence, commonly called *magnetic storms*, are found, when studied in their *mean* effects, to be governed by periodical laws of systematic order and regularity, and to exhibit periods whose duration is, respectively, 1, a solar day; 2, a solar year; and 3, a period of about ten of our solar years, corresponding both in duration and in the epochs of maximum and minimum variation, to the approximately decennial period discovered by Schwabe in the phænomena of the solar spots. In the present paper the author communicates the results of a similar investigation into the laws of the disturbances of the two other magnetic elements at Toronto, namely, the Inclination and the Total Force, derived from the hourly observations of the horizontal and vertical Forces during the five years from July 1843 to June 1848; affording, as he states, a full confirmation of the existence of periodical laws regulating the disturbances of the Inclination and Total Force corresponding to those which he had previously deduced from the disturbances of the other magnetic Element, viz. the Declination.

February 21, 1856.

The LORD WROTTESLEY, President, in the Chair.

The following communications were read :—

I. "On the Thermal Effects of Fluids in Motion." By Professor WILLIAM THOMSON, F.R.S., and J. P. JOULE, Esq., F.R.S. Received February 11, 1856.

A very great depression of temperature has been remarked by some observers when steam of high pressure issues from a small orifice into the open air. After the experiments we have made on the rush of air in similar circumstances, it could not be doubted that a great elevation of temperature of the issuing steam might be observed as well as the great depression usually supposed to be the only result. The method to obtain the entire thermal effect is obviously that which we have already employed in our experiments on permanently elastic fluids, viz. to transmit the steam through a porous material and to ascertain its temperature as it enters into and issues from the resisting medium. We have made a preliminary experiment of this kind which may be sufficiently interesting to place on record before proceeding to obtain more exact numerical results.

A short pipe an inch and a half diameter was screwed into an elbow pipe inserted into the top of a high pressure steam-boiler. A cotton plug placed in the short pipe had a fine wire of platina passed through it, the ends of which were connected with iron wires passing away to a sensitive galvanometer. The deflection due to a given difference of temperature of the same metallic junctions having been previously ascertained, we were able to estimate the difference of temperature of the steam at the opposite ends of the plug. The result of several experiments showed that for each lb. of pressure by which the steam on the pressure side exceeded that of the atmo-

sphere on the exit side there was a cooling effect of 0·2 Cent. The steam, therefore, issued at a temperature above 100° Cent., and, consequently, *dry*; showing the correctness of the view which we brought forward some years ago * as to the non-scalding property of steam issuing from a high pressure boiler.

II. "On the Bromide of Titanium." By F. B. Duppa, Esq. Communicated by A. W. Hofmann, Ph.D., F.R.S. &c. Received February 14, 1856.

A comparison of the boiling-points of corresponding chlorine and bromine compounds, led Prof. Kopp to the interesting discovery, that on the average their boiling-points rise 32° C for every equivalent of bromine which is substituted for an equivalent of chlorine.

	Boiling-point.	Difference.
Chloride of ethyl, $C_4 H_5 Cl$	11° C.	30.
Bromide of ethyl, $C_4 H_5 Br$	41° C.	
Dichlorinetted ethylene, $C_4 H_4 Cl_2$	67° C.	$66 = 2 \times 33$.
Dibrominetted ethylene, $C_4 H_4 Br_2$	133°·6C.	
Terchloride of phosphorus, $P Cl_3$.	78° C.	$97 = 3 \times 32\frac{1}{3}$.
Terbromide of phosphorus, $P Br_3$.	175° C.	

If this difference be constant for all chlorine and bromine compounds, it becomes obvious that very important inferences in respect to the atomic constitution of these substances may be derived from the determination of their boiling-points. This result has, in fact, been happily applied by Prof. Kopp, as a criterion to determine the equivalent of silicium, a matter of such uncertainty as to have led to the admission of not less than three formulæ for silica—

$$SiO$$
$$SiO_2$$
$$SiO_3.$$

From the difference between the boiling-points of chloride (59° C.)

* See letter from Mr. Thomson to Mr. Joule, published in the Philosophical Magazine, Nov. 1850.

and that of bromide (153° C.)—a difference which amounts to $94=3\times 31\frac{1}{3}$—Kopp derives the formulæ

$$SiCl_3 \text{ and } SiBr_3,$$

as representing the atomic constitution of the chloride and the bromide of silicium, and he accordingly fixes the equivalent of silicium at 21·3.

In order, however, to prove the general validity of Kopp's observations, it was necessary to re-examine the boiling-points of corresponding chlorine and bromine compounds which exhibited discrepancies, and to extend the inquiry to as great a number of new compounds as possible.

Mr. Francis Baldwin Duppa has, at my suggestion, undertaken an investigation of this subject, and has already obtained some valuable results, whieh I beg to communicate to the Royal Society.

The bromine-compound of titanium was unknown. Mr. Duppa has produced this substance by passing a current of bromine over an intimate mixture of pure titanic acid and carbon. The reaction takes place at a bright red heat, and furnishes a brown liquid, which solidifies in the receiver to a crystalline mass. Distilled with an excess of mercury, which removes any free bromine that may be present, the bromide of titanium presents itself as an amber-yellow compound, exhibiting a magnificent crystalline structure; it attracts moisture with the greatest avidity, and is converted into titanic and hydrobromic acid. Bromide of titanium has a specific gravity of 2·6. The fusing-point was found, 39°. The boiling-point was examined by Mr. Duppa with a considerable quantity of substance, the purity of which had been ascertained by analysis. It was observed to be 230° C. The boiling-point of the chloride of titanium, as observed by Dumas, and confirmed by Mr. Duppa, is 135°. The difference, $230-135=95=3\times 31\frac{1}{3}$, is exactly the same as that observed between the boiling-points of chloride and bromide of silicium.

This observation furnishes an additional support to the analogy of silicium and titanium, while it points unequivocally to the formulæ

$$TiCl_3 \text{ and } TiBr_3,$$

as representing the atomic constitution of these two compounds.

Titanic acid, hitherto universally represented as a binoxide TiO_2, would then assume the formula

$$TiO_3$$

in perfect analogy with that of silicic acid.

The equivalent of titanium would then be changed from 24·29, the number at present adopted, to 36·39. The protoxide of titanium would in this case become a sesquioxide, and the compound hitherto viewed as sesquioxide would have to be considered as an intermediate oxide—as a combination of the sesquioxide with the teroxide, in fact, as a bititanate of sesquioxide of titanium.

Formulæ of the Titanium Compounds.

Old Notation.		New Notation.
$Ti = 24 \cdot 29$		$Ti = 36 \cdot 39$.
$Ti\,O$	First oxide	$Ti_2\,O_3$
$Ti_2\,O_3$	Second oxide	$Ti_4\,O_9 = Ti_2\,O_3,\ 2TiO_3$
$Ti\,O_2$	Acid	$Ti\,O_3$
$Ti\,Cl_2$	Chloride	$Ti\,Cl_3$
$Ti\,Br_2$	Bromide	$Ti\,Br_3$.

It is scarcely necessary to observe, that an alteration of the equivalent of titanium on the ground of the difference of the two boiling-points, would be hazardous, if not supported by additional experimental evidence, and that further researches on the series of titanium are required in order to establish whether the proposed alteration actually affords a simpler expression for the combining relations of this remarkable element.

III. "Account of the Observations and Computations made for the purpose of ascertaining the amount of the deflection of the Plumb-Line at Arthur's Seat, and the Mean Specific Gravity of the Earth; with an account of the observed and computed amount of the Local Attraction at Arthur's Seat and at the Royal Observatory at Edinburgh." Communicated by Lieutenant-Colonel JAMES, R.E., F.R.S. &c., Superintendent of the Ordnance Survey. Received February 11, 1856.

(Abstract.)

Col. James begins by observing, that as the Royal Society has, from the very commencement of the Ordnance Survey of the United Kingdom, taken a deep interest in its progress, he has great pleasure in announcing to the Society that all the computations connected with the Primary Triangulation, the measurement of the Arcs of Meridians and the determination of the figure and dimensions of the earth are now completed, and that the account of all the operations and calculations which have been undertaken and executed is now in the press, and will shortly be in the hands of the public.

In the progress of these operations it has been found, on determining the most probable spheroid from all the astronomical and geodetic amplitudes in Great Britain, that the plumb-line is considerably deflected at several of the principal Trigonometrical Stations, and at almost every station the cause of the deflection is apparent in the configuration of the surrounding country.

The deflection of the plumb-line at Arthur's Seat is $5''\cdot 1$, and at the Royal Observatory at Edinburgh it amounts to $5''\cdot 63$ to the South. The unequal distribution of matter in the vicinity of these Stations—the great trough of the Firth of Forth being on the North and the range of the Pentland Hills on the South—presents an obvious cause for the deflection; but as the contoured plans of the county of Edinburgh have been published and the best attainable data acquired for estimating the amount of local attraction at the above-mentioned stations, it appeared desirable specially to investigate the matter, both on account of its scientific interest and with a

view to confirm the results arrived at from the previous investigation of all the observed latitudes. Col. James accordingly decided on having observations taken with Airy's Zenith Sector on the summit of Arthur's Seat, and at two other points near the meridian line on the North and South of that mountain, at about one-third of its altitude above the surrounding country.

The observations were made by Serjeant-Major Steel of the Royal Sappers and Miners, during the months of September and October last, and 220 double observations of stars were taken at each Station. The reductions and computations connected with these observations, as well as the computations of the local attraction at the Calton Hill, were entrusted to Captain Clarke, R.E., by whom the account now communicated of the mathematical investigation of the observed data has been drawn up.

Col. James has himself examined the geological structure of Arthur's Seat and of the whole of the county of Edinburgh, and has caused determinations to be made of the specific gravity of all the different rocks, with the view of estimating the mean specific gravity of the whole mass; but he observes, that although the geological structure of Arthur's Seat is well exposed, and its mean specific gravity, 2·75, has been employed for deducing that of the earth, viz. 5·1, still it is not a mountain he should have selected for this special object. Accordingly he was pleased by discovering—on referring, since these observations were made, to the correspondence of the Survey,—that the late Dr. Macculloch had been employed from 1814 to 1819 in examining the whole of Scotland for the purpose of selecting a mountain which, from its homogeneous structure, size, and form, would be best suited for the purpose referred to, and that he had pointed out the Stack Mountain in Sutherlandshire as admirably answering the required conditions. The transfer of the whole force of the Survey from the North of England and Scotland to Ireland, prevented the late General Colby from undertaking this investigation; but as the Survey of Scotland is now in full progress, Col. James purposes early in the spring to have the Stack Mountain and the surrounding country surveyed and contoured, and to have observations taken for determining the attraction of its mass, and he trusts by the close of the present year to lay the results before the Royal Society.

After these preliminary explanations, a detailed account is given of the mathematical investigation, and the paper, which was illustrated with plans and geological sections, and a model of Arthur's Seat, concludes with the following statement of the principal results :—

1. "The effect of the attraction of the Pentland Hills is observed in nearly equal amount at each of the three stations on Arthur's Seat.

2. "The calculated attractions of the mass of Arthur's Seat at the three stations are,—

South Station.	Arthur's Seat.	North Station.
$2''\cdot25$ North.	$0''\cdot34$ South.	$1''\cdot98$ South.

and, since the observed deflection at Arthur's Seat is $5''\cdot27$, the apparent effect of the Pentlands is $4''\cdot93$ at the summit of the hill.

3. "Of this deflection of $4''\cdot93$, the computed attraction due to the configuration of the ground within a radius of fifteen miles accounts for about $2''\cdot6$; and, inasmuch as we know that the igneous rocks of Arthur's Seat and the Pentland Hills have an origin at a great depth below the surface of the earth, the difference between the observed and computed attraction is probably owing in part to the high specific gravity of the mass of rock beneath them.

4. "The deflection at the Royal Observatory, Calton Hill, being $5''\cdot63$ South, exceeds that at Arthur's Seat by $0''\cdot70$. Of this deflection, $0''\cdot60$ is due to the configuration of the ground comprised within a circle of a mile and a quarter round the Observatory.

5. "The latitude of Arthur's Seat or points in the neighbourhood varies to the amount of $0''\cdot02$ between high and low water.

6. "The mean density of the earth, determined from the observations at the three stations on Arthur's Seat, is $5\cdot14$, with a probable error of $\pm\cdot07$ due to the probable errors of the astronomical amplitudes."

IV. "On some new Colouring Matters." By ARTHUR H. CHURCH and WILLIAM H. PERKIN. Communicated by A. W. HOFMANN, Ph.D., F.R.S. Received February 5, 1856.

Nascent hydrogen, acting upon an alcoholic solution of dinitrobenzole or of nitraniline, produces a crimson coloration, due to the formation of a new substance, to which we have given the name *nitrosophenyline*.

This new body presents some remarkable properties. It fuses below 100° C.; is uncrystallizable, and not volatile without decomposition; it dissolves in alcohol with an orange-red tint, and an alcoholic solution containing only ·2 per cent., although perfectly transparent to transmitted light, presents a flame-coloured luminous opacity in reflected light. Nitrosophenyline dissolves in hydrochloric acid, producing an intense crimson colour, which is changed to a yellowish-brown by alkalies and is restored by acids.

The analysis of nitrosophenyline has led us to the formula

$$C_{12}\,H_6\,N_2\,O_2,$$

which may be written thus:—$C_{12}{}^{H_6}_{NO_2}N$, and so may be viewed as aniline, in which 1 equiv. of hydrogen is replaced by 1 equiv. of binoxide of nitrogen: the following equation sufficiently explains the formation of nitrosophenyline:—

$$C_{12}\,H_4\,2NO_4+8H=C_{12}\,H_6\,N_2\,O_2+6HO.$$

We have produced from all the dinitro-compounds we have yet experimented upon, colouring matters similar to nitrosophenyline: the following is a list of such dinitro-compounds:—

1. Dinitrobenzole .... $C_{12}\,H_4\,2NO_4$.
2. Dinitrotoluole...... $C_{14}\,H_6\,2NO_4$.
3. Dinitroxylole ...... $C_{16}\,H_8\,2NO_4$.
4. Dinitroxylole ...... $C_{18}\,H_{10}\,2NO_4$.
5. Dinitrocymole...... $C_{20}\,H_{12}\,2NO_4$.
6. Dinitronaphthaline.. $C_{20}\,H_6\,2NO_4$.

We have examined minutely the colouring substance produced in

the case of dinitronaphthaline. It proves to be perfectly analogous in composition with nitrosophenyline; in properties also it is similar; and from its alcoholic solution it may be obtained in crystals, having a lustre somewhat similar to that of murexide: its formula, as deduced from our analysis, is

$$C_{20}\,H_8\,N_2\,O_2,$$

which we may arrange thus :—$C_{20}{}^{H_8}_{NO_2}\,N$, and so view it as naphthylamine in which 1 equiv. of hydrogen has been replaced by 1 equiv. of binoxide of nitrogen. This substance we term nitrosonaphthyline. It may likewise be obtained by the action of nitrous acid on naphthylamine, or of nitrite of potassium upon the hydrochlorate of naphthylamine: the following equations represent the three processes for its formation :—

1. $C_{20}\,H_6\,2NO_4+8H=C_{20}\,H_8\,N_2\,O_2+6HO.$
2. $C_{20}\,H_9\,N+NO_3=C_{20}\,H_8\,N_2\,O_2+HO.$
3. $C_{20}\,H_{10}\,N,\,Cl+KNO_4=C_{20}\,H_8\,N_2\,O_2+2HO+KCl.$

February 28, 1856.

The LORD WROTTESLEY, President, in the Chair.

The following communications were read :—

I. The following Letter was read, from Professor HANSTEEN of Christiania, For. Mem. R.S. :—

To the Royal Society of London.

As a Corresponding Member of the Royal Society, I have the honour herewith to transmit a Research "On the Secular Changes of the Magnetical System of the Earth, and more specially on the Secular Variation of the Magnetical Inclination in the Northern Temperate Zone," separately printed from the 'Memoirs of the Roy. Soc. of Sciences of Copenhagen.' By calculating newer and more ancient observations of the magnetical declination, I have ascertained the movement of the four magnetical polar regions, which I had

already found in my work 'Untersuchungen über den Magnetismus der Erde' (Christiania, 1819, with Atlas); whereof the two northern ones have a motion from west to east, the two southern ones in the contrary direction; and have attempted thereby in general to declare the cause of the known variations, as well of the system of declination as of that of inclination and of intensity.

As I am indebted for the greatest part of the materials to English observations, I have found it my duty to render my thanks to English science, and to express my hopes of future exertions towards the solution of this, in my thought, most interesting problem of the general physics of the globe.

Most respectfully,

CHRISTOPHER HANSTEEN.

Observatory in Christiania,
December 31, 1855.

II. THE BAKERIAN LECTURE.—"On the Electro-dynamic Properties of Metals." By Professor WILLIAM THOMSON, F.R.S.

The Lecturer gave an exposition of the substance of a paper presented by him to the Society under the above title.

The paper consists of five parts, namely:—1. On the Electric Convection of Heat; 2. On Thermo-electric Inversions; 3. On the Effects of Mechanical Strain and of Magnetization on the Thermo-electric Qualities of Metals; 4. On Methods for comparing and testing Galvanic Resistances, illustrated by Preliminary Experiments on the Effects of Tension and Magnetization on the Electric Conductivity of Metals; 5. On the Effects of Magnetization on the Electric Conductivity of Iron.

1. In the first part a full account of the experiments, of which the results were communicated to the Royal Society in April 1854*, is preceded by a short statement of the reasoning, founded on incontrovertible principles regarding the source of energy drawn upon by a thermo-electric current, which led the author to commence the experimental investigation with the certainty that the property looked for really existed whether he could find it or not. In confirmation of the extra-

* See Proceedings, May 4, 1854.

ordinary conclusion then announced,—that an electric current in an unequally heated conductor, if its *nominal direction* be from hot to cold through the metal, causes a cooling effect in iron, and a heating effect in copper,—the author describes new experiments which he has recently made, and which are as decisive in leading to the same conclusion as those by which he had first established it. He also describes experiments by which he had recently given an independent demonstration that brass has the same property as copper, and platinum the same quality as iron, with reference to electric convection of heat; results anticipated*, one as certain, and the other as highly probable, from the previous results regarding electric convection in copper and iron, and from the known thermo-electric relations between these metals and the others.

2. The phenomenon of thermo-electric inversion between metals, discovered by Cumming, forms the subject of the second part. A mode of experimenting is described, by which inversions may be readily detected when they exist between any two metals, and, when thermometers are available, the temperature of neutrality determined with precision. Various results of its application are mentioned, of which some are shown in the following Table:—

−14° Cent.	−12°·2	−1°·5	8°·2	36°	38°	44°	44°	47°
P_3 Brass	P_1 Cadmium	P_1 Silver	P_1 Zinc	P_2 Lead	P_2 Brass	P_2 Tin	Lead Brass	Silver Zinc

53°	64°	71°	72°	99°	121°	130°	162°·5	237°	280°
P_2 Double wire of Palladium, 11·31 grs., and Copper, 19·41 grs.	P_1 Copper	Silver Gold	Gold Zinc	P_1 Brass	P_1 Lead	P_1 Tin	Iron Cadmium	Iron Silver	Iron Copper

The number at the head of each column expresses the temperature Centigrade by mercurial thermometers, at which the two metals written below it are thermo-electrically neutral to one another; and the lower metal in each column is that which passes the other from *bismuth towards antimony as the temperature rises.* P_1, P_2, P_3 denote three particular specimens of platinum wire, used by the author as standards.

* See Proceedings, May 4, 1854; also "Dynamical Theory of Heat," Part VI. § 135; Transactions R.S.E., May 1854, p. 146.

It was also found that Aluminium must be neutral to either P_3, or Brass, or P_2, at some temperature between $-14°$ C. and $38°$ C.; that Brass becomes neutral to Copper at some high temperature, probably between 800° and 1400°; Copper to Silver, a little below the melting-point of silver; Nickel to Palladium, at some high temperature, perhaps about a low red heat; and P_3 to impure mercury (that had been used for amalgamating zinc plates), at a temperature between $-10°$ and $0°$. Probably P_3 becomes neutral to pure mercury at some temperature below $-10°$ C.

3. In the third part, effects of mechanical strain, and of magnetization on the thermo-electric qualities of metals, are investigated. The author had previously communicated to the Royal Society* results he had obtained regarding the thermo-electric qualities of copper and of iron wires under longitudinal stress, namely, that the former exhibits a deviation towards bismuth, and the latter towards antimony, from the same metal in an unstrained state.

The only kind of stress applicable to a solid which has no directional attributes, is uniform pressure or traction in all directions. Hence it appeared probable to the author that a simple longitudinal stress would induce different thermo-electric qualities in different directions, in any homogeneous non-crystalline metal subjected to it. But he had found (see Proceedings, May 4, 1854) that the thermo-electric effect of longitudinal traction on a wire, either of iron or of copper, is sensible to tests he could readily command, and more so in the case of the former than in that of the latter. He therefore made experiments to test the difference of thermo-electric quality in different directions in a mass of iron under stress, and fully established the conclusion that the thermo-electric quality across lines of traction differs from the thermo-electric quality along lines of traction, as bars of bismuth differ from bars of antimony. The experiments he has already made nearly establish the conclusion that unstrained iron has intermediate thermo-electric quality between those of the two critical directions in iron under distorting stress.

The experiments of Magnus show that wires hardened by wire-drawing have different thermo-electric qualities lengthwise from wires of the same substance softened by annealing. The author has veri-

* April 1854. See Proceedings, May 4, 1854.

fied, that in copper, iron, and tin, simple traction, leaving permanent elongation, leaves also a thermo-electric effect, the same as Magnus had found by wire-drawing, which is a composite application of longitudinal traction and lateral compression; and that in a variety of metals, namely, iron, copper, brass, tin, platinum, permanent lateral compression (by hammering) leaves still the same thermo-electric effect, as Magnus had found by wire-drawing. In cadmium, not examined by Magnus, and lead, which had not a given result, the experiments now adduced show a thermo-electric effect of hammering, the same as in all the other metals except iron. Zinc wire was also tested, and found to exhibit the same effect as copper, though Magnus had found a reverse quality as due to wire-drawing. The discrepance in this case is probably due to the peculiar effect of annealing on zinc wire, making it brittle and crystalline, which might give a different condition, as the "annealed" in Magnus's experiment, and the "unhammered" in the experiment now adduced. Setting aside this case, the author concludes that generally the effect of permanent lateral compression is the same as that of permanent longitudinal extension, or of hardening by wire-drawing, upon the thermo-electric quality of a wire placed longitudinally in an electric circuit; that in iron it is a deviation from the constrained metal towards bismuth, and that in all the other metals mentioned it is a deviation towards antimony; and that in copper and iron it is the reverse of the effect experienced by the same metal while under the stress that caused the strain. Since no kind of strain, except uniform condensation or dilatation in all directions, is free from the directional attribute, it appeared probable to the author that the thermo-electric effects remaining in a metal left with a longitudinal strain, retained after the stress that caused it is removed, must be different in different directions. He therefore experimented on iron hardened by longitudinal compression, and found that it deviates from soft iron towards antimony, or in the contrary way to iron hardened by longitudinal traction. From this, and from the results quoted above, it follows that in iron hardened by compression in one direction, the thermo-electric qualities in this direction differ from those in lines perpendicular to it, as antimony differs from bismuth; that the reverse statement applies to iron hardened by traction in one direction; and that these differing thermo-electric qualities have in

each case the thermo-electric quality of soft iron intermediate between them.

These various results show that the character of the effect in each case is decided by *distorting stress* or by *distortion*, and leave entirely open, and only to be answered by further experiments, the questions: what is the thermo-electric effect of pressure or traction, applied uniformly in all directions to a metal? and what is the thermo-electric effect of a permanent condensation or dilatation remaining in the metal, when freed from the force by which that condensation or dilatation was produced?

Experiments are also described, by which the author found that in soft iron under magnetic force, and in that retaining magnetism, when removed from the magnetizing force, directions along the lines of magnetization deviate thermo-electrically towards antimony; and that directions perpendicularly across the lines of magnetization in soft iron, deviate towards bismuth, from the unmagnetized metal. He illustrates this conclusion by an experiment on a riband of iron, magnetized nearly at an angle of 45° to its length, and heated along one edge while the other is kept cool. When the two ends, kept at the same temperature, are put in communication with the electrodes of a galvanometer, a powerful current is indicated, in such a direction, that if pursued along a rectangular zigzag from edge to edge through the band, the course is always *from across to along the lines of magnetization through the hot edge, and from along to across the lines of magnetization through the cold edge.*

4. In this part of the communication, attempts made by the author to find the effects of various influences on electric conductivities of metals are described. One of these, with a very unsatisfactory method for testing resistances, led to the conclusion that longitudinal magnetization diminishes the conducting quality of iron wire. The general plan for testing resistances, which he subsequently adopted as the best he could find, and which has proved very satisfactory, is next explained; and as an illustration, a single experiment on the relative effect of an equal longitudinal extension on the resistances of iron and copper wires is described. The conclusion established by this experiment is, that both by extension with the tractive force still in operation, and by permanent extension retained after a cessation of stress, the conductivity of the substance is more diminished

in iron than in copper ; or else that it is more increased in copper than in iron, or increased in copper while diminished in iron, if it is not in each metal diminished, as the author is led by a partial investigation of the absolute effect in each metal to believe.

5. The result previously arrived at regarding the effect of longitudinal magnetization on the conductivity of iron is confirmed ; and an experiment that would have been found impracticable by the less satisfactory method, proves the same conclusion for magnetized steel wire, with the magnetizing influence away. Two very different experiments show further, that the electric conductivity of magnetized iron is greater across than along the lines of magnetization. A last experiment, showing that iron gains in conducting power by magnetization across the lines of the electric current, leads to the conclusion that there is a direction inclined obliquely to the lines of magnetization, along which the conductivity of magnetized iron would remain unchanged on a cessation of the magnetizing force.

March 6, 1856.

Colonel SABINE, R.A., V.P. and Treasurer, in the Chair.

In accordance with the Statutes, the Secretary read the following list of Candidates for election into the Society :—

John Hutton Balfour, M.D.
Henry Foster Baxter, Esq.
Lionel Smith Beale, Esq.
Samuel Husbands Beckles, Esq.
Charles Tilstone Beke, Esq.
Edward W. Binney, Esq.
Sir John Bowring.
Edward Mounier Boxer, Capt. R.A.
Samuel Brown, Esq.
George Bowdler Buckton, Esq.
Sir John Fox Burgoyne, Bart.
William Coulson, Esq.
Thomas Russell Crampton, Esq.
Richard Cull, Esq.
Hugh Welch Diamond, M.D.
James Dixon, Esq.
Sir Charles Fox.
Philip Henry Gosse, Esq.
Robert Harkness, Esq.
Cæsar Henry Hawkins, Esq.
Dr. Humphreys.
Manuel John Johnson, Esq.
Edward Joseph Lowe, Esq.
Robert Wilfred Skeffington Lutwidge, Esq.
George Macilwain, Esq.
David Macloughlin, M.D.
William Marcet, M.D.
John Carrick Moore, Esq.
Robert William Mylne, Esq.
Henry Minchin Noad, Ph.D.
Edmund Potter, Esq.
Rev. T. Romney Robinson, D.D.
Henry Hyde Salter, M.D.
William Scovell Savory, Esq.
Archibald Smith, Esq.
Robert Angus Smith, Esq.
Thomas A. B. Spratt, Capt. R.N.
Henry Ward, Capt. R.E.
Thomas Williams, M.D.
Forbes Benignus Winslow, M.D.

The following communications were read :—

I. Supplement to the "Account of Pendulum Experiments undertaken in the Harton Colliery;" being an Account of Experiments undertaken to determine the correction for the Temperature of the Pendulum. By G. B. AIRY, Esq., Astronomer Royal. Received February 13, 1856.

(Abstract.)

Adverting to the circumstance that, in the Harton Experiment, there was a mean difference of 7° between the temperature above and below, and that a careful determination of the coefficient for temperature-correction was therefore necessary, the author describes the process by which the correction was now investigated by experiment on the same pendulums which were used in the Harton Experiment. Two rooms were selected at the Royal Observatory, Greenwich, having firm stone floors, and admitting of being heated, one by a stove in the room, the other by a hot-air-apparatus below. One pendulum was mounted upon its iron stand, with clock and other apparatus, in one room, and the other in the other room. Care was taken that the pendulums and their thermometers should be effectually protected from radiation. The two clocks were compared by carrying a chronometer from one to the other, and remarking the time of coincidence of beats; a method which admits of very great accuracy, when (as in this instance) the distance through which the chronometer is to be carried is small. In the Fifth Series (counting the series in sequence to those of the Harton Experiment), Pendulum 1821 was kept in heat, and Pendulum 8 cool, and continuous observations were kept up during forty hours. In the Sixth Series, Pendulum 8 only was kept in heat, and observations were again kept up during forty hours. The Seventh and Eighth Series were similar, respectively, to the Fifth and Sixth. The temperatures are referred to two of the thermometers used in the Harton Experiment, and to two other thermometers supplying the place of two of the Harton thermometers which cannot be found. The observations were con-

ducted entirely by Messrs. Dunkin and Ellis, Assistants of the Royal Observatory.

On discussing the results of the observations, there appears to be reason for supposing that a change has taken place in one of the pendulums after the Seventh Series. This appears from the circumstance that, though the Fifth and Seventh Series agree well, the Sixth and Eighth are discordant; and also from this circumstance, that the abstract relation between the two pendulums given by the Fifth, Sixth, and Seventh Series, agrees closely with that found at Harton; but if the Eighth Series is included, there is a considerable discordance.

If the Eighth Series is rejected, it appears that Colonel Sabine's coefficient ought to be increased by about $\frac{1}{28}$th part; and on introducing this correction into the computations of the Harton Experiment, the result for the earth's mean density is 6·809. If the Eighth Series is retained, the correction is reduced to less than one-fourth of that just mentioned, and the earth's mean density is 6·623.

The author subjoins an investigation with which he has been favoured by Professor Stokes on the effect of the rotation and ellipticity of the earth in modifying the numerical results of the Harton Experiment. It appears that the numbers found in the paper ought to be multiplied by

$$1+m-\frac{\epsilon}{2}+\frac{3}{2}\epsilon\cos 2l,$$

$$\text{where } m=\frac{\text{equatoreal centrifugal force}}{\text{gravity}}$$

$$\epsilon=\text{ellipticity}$$

$$l=\text{latitude of place.}$$

On converting this formula into numbers, for Harton, the factor is found to be 1·00012, which produces no sensible change in the result.

At the equator the factor would have been 1·00679.

II. "On the Mathematical Theory of the Stability of Earthwork and Masonry." In a Letter to Prof. STOKES, Sec. R.S. By W. J. MACQUORN RANKINE, Esq., C.E., F.R.S., Professor of Civil Engineering in the University of Glasgow. Received February 19, 1856.

In the preparation of my course of lectures, I have found it necessary to re-investigate much of the above-named branch of mechanics, and I have now a paper in preparation on the subject, which I propose to offer to the Royal Society when it is ready. In the meanwhile, it appears to me that the two fundamental principles on which my researches are based are of such a nature, that they may very properly be communicated to the Royal Society at once. They are as follows:—

I. *Principle of the Stability of Earth.*

At each point in a mass of earth the directions of greatest and least compressive stress are at right angles to each other; and the condition of stability is, that at each point the ratio of the difference of those stresses to their sum shall not exceed the sine of the angle of natural slope of the earth.

II. *Principle of the Transformation of Structures.*

Let a structure of a given uniform transverse section be stable under a system of forces represented by given lines in the plane of section:—Then will any other structure whose transverse section is a *projection by parallel lines* of that of the first structure upon any other plane, be stable under the system of forces represented by the projections, upon the new plane, of the lines representing the first system of forces.

Example of the application of this principle.

Let fig. 1 be an equilibrated arch with its abutments of the form (for example) proposed by M. Yvon-Villarceaux, suited for a horizontal extrados EF. OK, OA, and AB being given, all the dimensions of the arch and abutments are functions of those three quantities.

It is required to design an arch, fig. 2, for an extrados *ef*, at any given inclination, of any given span *cd* (measured parallel to the ex-

trados), and in which ok=OK, oa=OA, and ab=AB, are the same as in the primitive arch fig. 1.

Fig. 1.

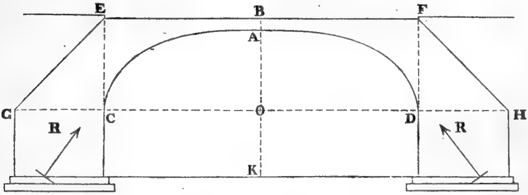
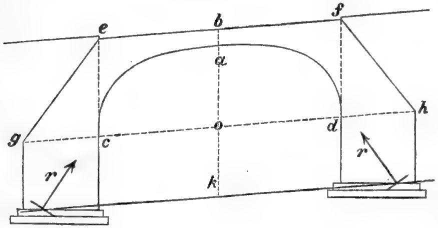


Fig. 2.



Solution. On any vertical plane passing through BK, and not coinciding with the plane of fig. 1, draw cod of the given length and inclination, intersecting COD in O. Join Cc, Dd, and project the whole of fig. 1 on the new plane by lines parallel to Cc, Dd. The projection so obtained will be the figure of the arch and abutments required. Moreover, if the lines R, R, fig. 1, represent in length, direction, and position, the resultants of the pressures of the abutments on their foundations in the original arch, then will r, r, fig. 2, the projections of R, R, represent the corresponding resultants in the new arch; and in like manner, the thrust at a is the projection of the thrust at A.

W. J. Macquorn Rankine.

Glasgow, 18th February, 1856.

Note. The horizontal foundation courses in fig. 2 do not form part of the projection of fig. 1, but are supposed to be added after the completion of the projection.

III. Letter from JAMES P. JOULE, Esq. F.R.S. to Prof. STOKES, in reference to the Paper of Dr. WOODS read on the 10th of January 1856. Received February 22, 1856.

Manchester, February 21st, 1856.

In the abstract of Dr. Woods' paper printed in the 'Proceedings' for January 10th, the following remark occurs: "Mr. Joule published in the Philosophical Magazine for June 1852, a memoir proving exactly the same proposition, but giving me the merit of priority in a preliminary remark." In justice to myself I must state that my actual words were—"I observe with pleasure that Dr. Woods has recently arrived at one of the results of the paper, viz. 'that the decomposition of a compound body occasions as much cold as the combination of its elements originally produced heat,' by the use of an elegant experimental process described in this Magazine for October 1851. I ought, however, to remark, that previous to the year 1843 I had demonstrated 'that the heat rendered latent in the electrolysis of water is at the expense of the heat which would otherwise have been evolved in a free state by the circuit.'"

The memoir referred to by Dr. Woods was acknowledged by the French Academy in its 'Comptes Rendus' for Feb. 9, 1846, and according to established rule dates from that period. I may however observe that the law he claims was published by me in the Philosophical Magazine for October 1841, where I pointed out that the heat evolved by the combination of oxygen and hydrogen is equal to that due to the electrical intensity required to separate water into its elements. The same fact was reiterated in various subsequent papers, in which it is also proved that "the quantities of heat which are evolved by the combustion of the chemical equivalents of bodies are proportional to the intensities of their affinities for oxygen" (Phil. Mag. xx. p. 111), a proposition which is given as his own by Dr. Woods, and considered by him as "an original idea."

JAMES P. JOULE.

March 13, 1856.

Sir BENJAMIN C. BRODIE, Bart., V.P., in the Chair.

The following communications were read:—

I. "On the presence of fibrils of soft tissue in the Dentinal Tubes." By JOHN TOMES, Esq., F.R.S. Received February 21, 1856.

(Abstract.)

Referring to the structural characters of dentine, and to the prevailing belief that the dentinal tubes in the normal condition contain fluid, the author goes on to show that the recognized histological characters fail to account for the high degree of sensibility exhibited by the dentine when diseased, or when suddenly exposed by the removal of the enamel.

It is found, moreover, that the dentine is not uniformly sensitive throughout, but possesses a much higher degree of sensibility at the peripheral distribution of the dentinal tubes than deeper in the substance of the tooth; and it is urged that these facts cannot be accounted for by the presence of a fluid in the dentinal tubes, nor by supposing that the hard unyielding dentine is intrinsically endowed with sensation. This view of the matter is borne out by the fact, that all sensibility is at once lost if the pulp of the tooth be destroyed.

Finding that the dentine owed its sensibility to the presence of the dentinal pulp, and knowing that the tubes have open extremities in contact with the pulp, the author was induced to examine carefully the contents of the tubes. The investigation resulted in discovering that the dentinal tubes, instead of containing fluid only, give passage to fibrils of soft tissue, which pass from the pulp into the tubes where these open upon the surface of the pulp-cavity, and from thence may be traced into the branches. The fibrils may be demonstrated by fracturing a perfectly fresh tooth, and then with a sharp knife taking very thin sections from the dentine near the edge of

the pulp-cavity. The dentine will, when cut, break up into small fragments, and from the edges of these the fibrils may be seen extending. Sometimes a small portion of the pulp will be found adherent, in which case the fibrils may be seen to extend from that tissue into the dentine. The fibrils may be shown in a more striking manner by decalcifying a section, and then, when it is placed upon a slide, tearing the specimen across the direction of the tubes. By this manipulation, the fibrils will be dragged out from one fragment, and will be seen projecting from the edge of the other.

The fibrils, when isolated and examined with a high power, without the presence of a reagent, show some indications of tubularity, but not with sufficient distinctness to enable the author to determine whether they are tubes or solid bodies. Their appearance is very like that of the ultimate fibrils of spinal nerves, and they possess a character in common with these, in the presence of minute globules of dense transparent matter exuded from the broken ends, and sometimes from the surface of the fibril. It is not easy to determine in what manner the fibrils commence in the pulp. In some preparations they appear to be connected with cells situated a short distance within the pulp, in others they may be traced to a greater depth, where they are lost in the tissue of the pulp, and may possibly be connected with the nerves, which in this part are very abundant. But in the absence of exact knowledge as to the manner in which the dentinal fibrils are related to the elements of the pulp, the author considers that there is sufficient evidence to warrant the conclusion that they are organs of sensation, the distribution of which through the substance of the dentine endows that tissue with its sensibility.

This conclusion is borne out by the occurrence of the following conditions. If a fragment of enamel be broken from the surface of the dentine, the exposed portion of the latter tissue is highly sensitive to the contact of foreign bodies; but if the force producing the injury be sufficient to rupture the nerves and vessels where they enter the root of the tooth, the dentine loses its capability of feeling pain. Again, if the dentine be exposed by the gradual wearing away of the enamel by mastication, the surface evinces no sensibility, a circumstance accounted for by the fact that the dentinal tubes have become consolidated, either at the surface exposed, or at some point between the surface and the pulp-cavity. Diseased teeth furnish

further evidence in favour of the foregoing views. If a carious tooth, in which the disease has advanced but slowly, and the carious portion is dark in colour and tolerably firm in consistence, be examined, it will be found that the dentinal fibrils have become calcified, and in a favourable section they may be seen projecting from the edge of the specimen, or lying broken in short lengths in the tubes. On removing the diseased part of the tooth in such a case, no pain is experienced until the instrument reaches the healthy dentine. Supposing, however, the disease to have been rapid in its progress, the carious tissue will be light in colour, and, as compared with the preceding example, soft in consistence. The removal of the affected part in this case is frequently attended with considerable pain. Examination will then show that the dentinal fibrils have not been consolidated, but may be found here and there extending into the softened tissue without having suffered any appreciable alteration of appearance.

Daily experience shows that a tooth may remain useful for a long time after the pulp, and consequently the dentinal fibrils, have been destroyed. If, however, a tooth which has been so circumstanced be examined, it will be found that one of two actions has been set up. Either additional cementum will have been developed upon the surface of the fang, or its bulk will have become diminished by absorption. Similar conditions supervene when the crown of a tooth has been lost by caries.

In old persons we find the teeth are lost without apparent disease in the dental tissues. The teeth become loose and fall out, the roots being in such cases translucent like horn. This condition is the result of consolidation of the dentinal fibrils, and is followed by absorption of the cementum and dentine. Cases may be found in which the whole of the fang has been absorbed, but reduction to two-thirds or half of the normal bulk is very common.

The concurrence of the foregoing changes in the sensibility of the tooth, with the destruction or consolidation of the dentinal fibrils, will, the author considers, justify the conclusion, that the dentinal fibrils, in a state of integrity, are necessary to the normal condition of dentine.

II. "On the Structure and Development of the Cysticercus Cellulosæ, as found in the Pig." By GEORGE RAINEY, Esq., Lecturer, and Demonstrator of Practical and Microscopical Anatomy at St. Thomas's Hospital. Communicated by ROBERT D. THOMSON, M.D., F.R.S. Received February 15, 1856.

The present communication is an enlarged and revised version, illustrated with figures, of a paper by the author, bearing the same title, which was read on the 13th December 1855, and was subsequently, by permission, withdrawn. An abstract will be found in the 'Proceedings' under the date referred to.

III. A paper was in part read, entitled "On the Diœcious Character of the Rotifera." By PHILIP HENRY GOSSE, Esq. Communicated by THOMAS BELL, Esq., F.R.S., Pres.L.S. Received February 19, 1856.

The Society then adjourned over the Easter holidays, to Thursday, April 3.

April 3, 1856.

Sir PHILIP DE MALPAS GREY EGERTON, Bart., V.P., in the Chair.

The reading of Mr. Gosse's paper, "On the Diœcious Character of the Rotifera," was resumed and concluded.

(Abstract.)

Professor Ehrenberg, in his descriptions of this class of animals, assumed them to be in every case hermaphrodite. His conclusions remained unchallenged till 1848, when Mr. Brightwell discovered

the separate sexes of *Asplanchna Brightwellii.* The author of this memoir soon afterwards discovered a second species of the same genus (*A. priodonta*) with a like diœcious character; and more recently Dr. Leydig has added a third (*A. Sieboldii*), which does not differ in this respect from its congeners.

Dr. Leydig plausibly conjectures that *Enteroplea* of Ehrenberg is the male sex of *Hydatina*, that *Notommata granularis* is the male of *N. Brachionus*, and that *Diglena granularis* of Weisse is the male of *D. Catellina.*

The author of the present memoir has ascertained from his own observations that the sexes are separate also in *Brachionus Pala, B. rubens, B. amphiceros, B. angularis, B. Bakeri, B. Dorcas, B. Mülleri, Synchæta tremula, Polyarthra platyptera, Sacculus viridis,* and *Melicerta ringens.* The males of these species, which are here described in detail, differ so greatly from the females in form, size, and structure, that they could not have been supposed to belong to the same genera, or even families, if their parentage had not been distinctly determined.

One of the most remarkable characters of male Rotifera is the absolute and universal atrophy of the digestive system. No mastax, jaws, œsophagus, stomach, or intestines occur in any example of any species. Another peculiarity is the great disparity between the sexes. In every observed case the male is inferior in size and in organization to the female.

The muscular system is well developed in the males of *Hydatina, Asplanchna,* and *Brach. Mülleri.* The frontal cilia are in general greatly developed in this sex, the result of which is seen in the energy and rapidity of its locomotion. In most instances the great occipital ganglion is distinct, with a red eye seated on it; and the latter is almost always present, even where the ganglion cannot be defined. The lateral convoluted threads appear in *Hydatina, Asplanchna,* and *Brach. dorcas*; and in *Aspl. Brightwellii* they are accompanied by tremulous tags, and by a contractile bladder.

Irregular masses of opake substance are almost constantly present in male Rotifera. This substance Dr. Leydig considers a urinary concretion.

In all cases the abdominal cavity is occupied by a capacious sperm-sac, from which spermatozoa are forced out by pressure. The out-

let of the sperm-sac is by a thick, protrusile, and retractile penis. In those species which possess a foot, the intromittent organ is soldered to its dorsal side, and is often so greatly developed that the foot itself appears as an appendage. The penis is protruded by eversion; and is then seen to be a thick column with the extremity truncate and ciliated. The sexual coitus has been witnessed by the author in several instances.

For a parallel to the curious facts thus established, the author considers we must look to the Crustacea. The *Hectocotylus* of certain Mollusca is scarcely an analogous case; nor are those Entozoa in which the males are organically united to the females.

In the Crustacea, however, many examples occur of a sexual difference which may be compared with that of the subjects of this memoir. In the genera *Bopyrus, Phryxus,* and *Ione,* the males are notably smaller than the females, very diverse in form, and in some respects inferior in structure. In the *Siphonostoma,* "the males are extremely small, and do not *in the least* resemble the females" (Baird); though those of different genera bear a strong resemblance *inter se,* even where the females are very dissimilar. So low is their grade of organization, that Burmeister has attempted to prove the minute males to be embryonic forms. Finally, in the Cirripedia, Mr. Darwin has proved the existence of males in the genera *Ibla* and *Scalpellum,* which are very minute as compared with their females, excessively abnormal in form, and in some respects in an embryonic condition, though unquestionably mature, as shown by their spermatozoa. And, what is still more interesting, there is, in these male Cirripedia, "no vestige of a mouth, or masticatory organs, or stomach." The same observer describes the internal structure as "a pulpy mass with numerous oil-globules;" and the sperm-vesicle as "a pear-shaped bag at the very bottom of the sack-formed animal containing either pulpy matter, or a great mass of spermatozoa,"—terms which might have been employed in describing some of the male *Brachioni.*

In all these analogies, the author finds additional reasons for assigning to the Rotifera a zoological rank among the Articulata.

April 10, 1856.

Colonel SABINE, R.A., V.P. and Treasurer, in the Chair.

The following communications were read:—

I. "Account of Experiments on the Vagus and Spinal Accessory Nerves." By AUGUSTUS WALLER, M.D., F.R.S. Received March 31, 1856.

(Abstract.)

The important functions of the organs more or less completely dependent for their innervation on the vagus nerve, have afforded the reason of so many attempts by previous physiologists to determine the exact influence exerted by the fibres arising from different sources which are intimately blended together in the trunk of the mixed vagus. Since Sir Charles Bell's discovery of the different functions of the anterior and posterior roots of the spinal pairs, it has become still more important to determine how far the same law holds good with regard to the vagus nerve, and whether at its origin, it is a purely sensory nerve, receiving its motor fibres from the internal branch of the spinal accessory and perhaps from other sources. According to Bischoff and Longet, the vagus at its origin and as far as the upper ganglion, is purely sensory, and becomes possessed of motor power from its junction with the internal branch of the accessory, and from other branches derived from motor nerves (Longet). The observations of Bernard have led him on the contrary to adopt the opinion, that the vagus at its origin is a mixed nerve; because after destroying the spinal accessory, no effect on the functions of the heart, stomach, or lungs was observed, and the only organs visibly dependent on the spinal accessory were the larynx and pharynx. After bearing testimony to the correctness of the observations made by Bernard with regard to the effects immediately following destruction of the spinal accessory, the author considered it desirable to apply to the determina-

tion of this question his new means of investigation, in addition to those previously employed. His first observations on the vagus, published in the 'Comptes Rendus' of the Académie des Sciences, 1852, had already led him to entertain the idea, that the vagus proper was a purely sensory nerve. They first consisted in cutting this nerve between its upper and lower ganglia, the section comprising the internal branch of the spinal accessory. After the animal had been kept alive for a sufficient period to cause disorganization of the nerve fibres, the central and distal portions were examined microscopically. In the central portion, the fibres of the internal branch of the spinal accessory included in the section were all in a sound state; some of the fascicles of origin of the vagus were also sound; the remaining fascicles of origin of the vagus consisted of disorganized fibres. The analogy existing between the results as to the last-mentioned fibres, and those which follow section of the sensory roots of the spinal nerves, led the author to the conclusion, that the disorganized fibres of the fascicles of the vagus were likewise sensory, and had their trophic centre in its lower ganglion, while the sound fibres probably had their trophic centre in the upper ganglion. On these grounds, and some others affording concurrent testimony, he concluded that the vagus in itself was probably a purely sensory nerve. In the distal end, the part below the second ganglion consisted likewise of a mixture of sound and altered fibres. When tested by galvanism on the living animal, it was found that the nerve had lost all power of exciting movement in the various organs which it supplies, and that such branches of known motor power as the recurrent and the crico-thyroid were disorganized in their structure. It was evident therefore that the lower ganglion did not arrest the disorganization of the motor fibres contained in the vagus, because, as has been proved by the author in other cases, when the disorganization of the motor fibres of nerves distributed to muscles is arrested,—as by the superior cervical ganglion of the sympathetic nerve, or by the effect of hybernation in the frog,—the motor power of these nerves is retained.

In the present observations, the accessory nerve was divided at its origin by Bernard's process of evulsion of its roots, and after the lapse of about a fortnight the vagus was tested by galvanism, and examined by the microscope, in order to ascertain the functions

which it had lost, and the fibres which were disorganized, the sound vagus on the opposite side being taken as a standard of comparison. On the sound side, galvanism of the vagus caused each time strong dilatation of the glottis by the retraction of the corresponding arytenoid cartilage. On the operated side, galvanism produced a slight movement of the glottis on the same side by drawing inwards of the arytenoid.

On the heart, the action of galvanism of the sound vagus was manifest by the stoppage of the pulsation of the carotid arteries and the diminution of their calibre. On the operated side, no influence on the pulsation of the vessels was observed by galvanizing the vagus. When the heart was exposed by removing a portion of the thorax, and keeping up artificial respiration, galvanism on the sound vagus produced complete stoppage of the heart's action, while on the opposite side, irritation of the vagus exerted no influence on the heart.

The stomach being exposed was found distended with food. Galvanism of the sound vagus caused evident contractions of this organ, which were strongest at the neck or constriction which it usually presents (in the rabbit),—from whence they radiated in both directions, becoming more and more faint. On stimulating the other vagus, from which the accessory had been virtually eliminated, no perceptible influence was observed.

Microscopic examination showed that the cervical part of the vagus of the side operated on contained numerous disorganized fibres, almost all collected together in a single fasciculus, which included only a few normal fibres. In the recurrent branch were disorganized fibres, corresponding very closely to those found in the vagus above it. Below the recurrent, the vagus and its cardiac, pulmonary and gastric branches consisted almost entirely of normal fibres, most of which, as is well known, are nucleated fibres.

From the foregoing observations, the author draws the conclusion, that from the spinal accessory are derived the greater part of the motor fibres contained in the vagus, which govern the movement of the larynx, the heart, and the stomach. He likewise infers from the microscopic examination of the vagus below the recurrent, that the motor fibres distributed to the heart and stomach belong almost exclusively to the nucleated or 'Remak' fibres.

The author purposes communicating in a future paper his researches on the other organs supplied by the vagus.

The above experiments were principally carried out in the laboratory of M. Flourens at the Jardin des Plants, who facilitated in every way the researches, and where the author had the able assistance of Drs. Philipeaux and Vulpian.

II. "Extract of a Letter to GEORGE RENNIE, Esq., F.R.S., from P. A. SECCHI, Director of the Astronomical Observatory of the Collegio Romano, containing explanatory remarks on a drawing of the Lunar Spot, 'Copernicus,' presented by him to the Royal Society. Dated Rome, March 13, 1856." Communicated by GEORGE RENNIE, Esq., F.R.S.

"As to the drawing of the spot of the moon, it is a first attempt to obtain an accurate representation of the interesting spot 'Copernicus.' In such large dimensions, photography directly taken with the telescope has been impossible; I therefore made first an accurate triangulation of the spot with the micrometer, and the principal points were thus laid down on the chart, after which operation the rest was filled in by the eye alone. The power used has been always either 1000 or 760. As it was impossible to carry through such a work in a single night, on the first night of good opportunity a general outline was taken, and on the other evenings particular drawings were made, and all these parts, taken in different grades of light and shadow, were afterwards harmonized together and compared with the moon when the point of light was seen to be the same as on the first night. So this work occupied more than six months, that is, all the favourable positions (two at each lunation) which could be obtained. I do not pretend it to be yet accurate enough to be transferred from photography* to any kind of engraving, but I am watching every good occasion to make it complete. But before bestowing more time and labour, I should be glad to know the impression such a work may make among the scientific men of England. I must observe that the most distant outliers of the crater have not been included," &c.

* The figure presented to the Society is a photographic copy of the original drawing.—(ED.)

III. "Notes on the Drawing of 'Copernicus,' presented to the Royal Society by P. A. SECCHI." By JOHN PHILLIPS, Esq., F.R.S. Communicated by Col. SABINE, V.P. & Treas. R.S. Received April 9, 1856.

Of the few attempts which have been made of late years to prepare drawings*, on a large scale, of selected lunar mountains, this contribution from the Roman Observatory appears to be one of the most successful. It is on a scale of magnitude (about 10 geographical miles to one inch) such as only the larger modern telescopes can command, and characterized by such firmness of definition as to do honour alike to the maker of the instrument and to the artist engaged in the delineation. It may assist those who have not attempted, with their own hands, any drawings of this kind, and desire to form a right judgement of the value of this work of P. Secchi, if I send for comparison a drawing of Gassendi, executed from my object-glass of $6\frac{1}{4}$ inches (Cook), with a focal length of 11 feet. The drawing is on a scale of 20 geographical miles to an inch, and Gassendi thus appears of half the linear dimension of Copernicus, being really almost of the same diameter.

Placing together the two drawings, and remembering the appearance of Copernicus, as I have seen it through telescopes, some reflections arise which it may be permitted me to express, in the hope that we are now fairly entered on the long career of discoveries in the moon, to which the attention of astronomers has been of late systematically drawn by the Earl of Rosse and a Committee of the British Association.

In proportion as the power of the telescope rises, the seemingly simple 'ring mountains' of the moon exhibit as much diversity of outline and structure as the larger terrestrial volcanoes when accurately mapped. Thus while Gassendi,—40° from the central meridian of the moon, and 17° south† of the equator,—has the obliquely elliptical contour due to a circle in that position, Copernicus, 20°

* As distinct from mere *plans*. The drawings must however be based on exact plans.

† The Poles being named after the type of Mädler's noble work, 'Der Mond.'

from the central meridian, and 10° north of the equator, has its most conspicuous peripheral crest formed of seven principal nearly straight elements, approaching to equality in length, and meeting in points which are situated almost exactly in a circle of 24 geographical miles radius. Here is a very important partial difference, coupled with a very important general agreement.

While Gassendi, with peaks 9000 feet high, projects like a huge narrow wall into the Mare Humorum, and hangs over the interior plain in precipices as steep and many times as high as those over the Atrio del Cavallo, Copernicus, seated in the midst of broad land, on a base of 120 geographical miles, rises in many broken stages, bristling with a thousand silver-bright crests,—a perfect network of rough and complicated ground, crossed by lights and shades, which have a history of their own,—and toward the inside falls off by many irregular terraces, down to an interior plain, as if the whole area had yielded, and the surface had been formed by enormous land-slips. Four sharp notches are traced across the narrow ridge of Gassendi cutting it deeply, like the hollows left by decomposing lava dykes 500 feet broad; one deeper and broader opening unites the inner plain with the outer Mare Humorum, and one far wider opening leads to an accessory crater, over whose awful depth the cliffs, 10,000 to 12,000 ft. high, spread black shadows round some central rocks. In these particulars Copernicus offers a very different aspect. Its high crest, of 10,000 feet, is only cut through by one straight narrow meridional groove, though broken by numerous fissures in other parts, and is in all parts so irregular, partially undulated, and varied with small crateriform points, and enclosed areas, resembling craters, as to offer little analogy to any truncated cone of eruption. The highest summit, on the left hand (west) side—a huge rock—is conspicuous by its broad, deep and extended shade. What suggests a vast lava current, is equally remarkable on the northern slope. Regarding now the central plains of these mountains, we remark in each several low ridges of rather sinuous forms, and several small mounds (half a mile or more across), of which *three central digitated masses, not pierced by craters,* are the most elevated, and catch the earliest lights of morning which glance over the rocky borders of the basin. Had the drawings been executed at the

instant of sunrise on the central meridian line of the basin, these points would have stood up on the soft edge of the light and shade, as bright as the Swiss mountains at sunrise or sunset, but not like them reddened by the optical property of the atmosphere. Gassendi has *at least two* (I have somewhere a memorandum of more) small craters within the central plain. None such appear in this drawing of Copernicus. In many other lunar mountains the centre is occupied by a crater-formed hill, as Vesuvius stands within Somma; in others the hill remains a smooth rounded mass, but its crater is lost; and a further stage of decay seems to be seen in Gassendi and Copernicus, where the central mass is broken into fragments and sculptured by ramified hollows. May we ascribe these effects to the former action of a lunar atmosphere, now absorbed in the oxidated crust of the moon? If so, the lunar mountains have a history of water, as well as records of fire, and we must look on the sinuous ridges of the Mare Humorum with eyes accustomed to the gravel mounds of Norway and Ireland; study the degraded craters after the models of the Eifel; and map the 'rillen*' with reference to valleys of erosion as well as of eruption.

In questions of this kind we shall find such drawings as this of the Roman astronomer of priceless value. Studied, scrutinized, enriched with new discoveries, it may be the model for all time to come in this line of research. It may be followed by two other drawings of the same mountain,—one at the moment when the sun is on the meridian of the central hillocks, *to show the light streaks*, which hide themselves when the sun is low, and another in the clear afternoon of the lunar day (as much after midday, as this drawing was taken before noon), when every little crack and cavity becomes again distinct, *but greatly altered in aspect*, and the whole landscape changes under the eye of the observer; the plains growing grayer and softer, and revealing many minute low undulations; the hills looking more and more rugged, and burning with narrower, brighter and more angular tracts of silvery light.

* I have some curious results regarding these beautiful objects.

IV. "A Third Memoir on Quantics." By ARTHUR CAYLEY, Esq., F.R.S. Received March 13, 1856.

The object of the author in the present memoir is chiefly to collect together and put upon record various results useful in the theories of the particular quantics to which they relate. The tables at the commencement relate to binary quantics, and are a direct sequel to the tables in the author's second memoir upon Quantics, Phil. Trans. vol. cxlvi. (1856). The definitions and explanations in the next part of the present memoir are given here for the sake of convenience, the further development of the subjects to which they relate being reserved for another occasion. The remainder of the memoir consists of tables and explanations relating to ternary quadrics and cubics.

V. "Elementary Considerations on the subject of Rotatory Motion." By W. GRAVATT, Esq., F.R.S. Communicated by the Rev. J. B. READE, F.R.S. Received March 12, 1856.

(Abstract.)

The author explains the subject of rotatory motion in a series of propositions by the use of prime and ultimate ratios. He commences with a simple problem, determining the law of the forces by which a particle of matter is deflected into any given course, and pursues the inquiry by a consideration of the effect of these forces as referred to a sphere, going on to the investigation of the character of the motion of any body enclosed within an imaginary sphere, such sphere itself being supposed to revolve upon two axes inclined at any angle to each other. Hence the author determines the position of some point of the circumscribing sphere momentarily at rest, or in other words, of the *resultant axis*, from which he insists that all centrifugal forces must really be calculated.

His first application of the law thus enunciated is to the motion of the peg-top; and upon the principles he has already laid down, he shows that there is in the first instance rotation round a momentary horizontal axis, calling up rotation round a momentary vertical axis;

and that the ratio of the velocities of these two rotations, together with the length of the peg, determines the angular inclination of the top, contrary to the received explanation as given by Euler and other mathematicians.

The law is further applied to the effect produced upon a falling body by the axial rotation of the earth, in the discussion of which, La Place, in the opinion of the author, has committed two important errors one in denying any deviation towards the equator, the other ; in his calculation of the amount of the deviation towards the east.

This is followed by an investigation of the motion or direction of flight of a cannon-ball or shell fired in a northerly or southerly direction, from which it appears that a large shell will be subject to a deviation from the true line of projection, in consequence of the earth's rotation, amounting to no less than 22 feet.

The author then refers to the well-known experiment of M. Foucault for proving sensibly the rotation of the earth, and shows from calculation that the errors which would be sufficient to vitiate the results in this experiment are so extremely minute and so difficult of avoidance by any perfection of manipulation which can be employed, that its performance cannot perhaps be safely adduced as proving such rotation.

The author illustrated his views by the exhibition to the meeting of a model apparatus, in which the vertical and horizontal motions may be variously combined, but which could not be intelligibly described without a series of complicated drawings unfitted for the compass of a mere abstract.

April 17, 1856.

The LORD WROTTESLEY, President, in the Chair.

The following communications were read :—

I. " On the Condition of the Oxygen absorbed into the Blood during Respiration." By GEORGE HARLEY, M.D., Teacher of Practical Physiology and Histology in University College, London. Communicated by Professor SHARPEY, M.D., Sec. R.S. Received March 16, 1856.

(Abstract.)

The author commences by explaining, that his researches were instituted with the view of ascertaining whether the doctrine maintained by Magnus in regard to the gases interchanged in the lungs during respiration were correct—namely, that the gases in question enter into *no* chemical combination with the constituents of the blood, either in passing to or from the tissues and organs of the body, but form merely a physical mixture with the circulating liquid. The principal object of the inquiry was to determine the following points :—

1. Has blood the property of chemically combining with the respired oxygen?

2. Which of the constituents of the blood enter into combination with oxygen?

3. Do these constituents, by combining with oxygen, simply become oxidized, or do they also yield carbonic acid gas?

4. What are the agents which control these changes?

After describing the method of investigation, and the apparatus employed, the author proceeds to relate a few of the analyses which he considered as the most conclusive. Instead of confirming the view of Magnus, that gases enter into no chemical combination with

blood, his results led him to conclusions of an opposite character, which serve to confirm the more generally received doctrine.

In one set of experiments a certain quaintity of fresh ox-blood was first shaken with renewed portions of air until it had become thoroughly saturated with oxygen, then introduced into a graduated glass vessel with 100 per cent. of ordinary air, corked carefully up, and kept during twenty-four hours in a room of moderate temperature. In order to favour the mutual action of the air and blood, the vessel was frequently agitated. At the expiration of twenty-four hours the gas was analysed by Bunsen's method. In an example cited the following was found to be its composition:—

$$\left.\begin{array}{ll}\text{Oxygen}\ldots\ldots\ldots & 10{\cdot}42\\ \text{Carbonic acid}\ \ldots. & 5{\cdot}05\end{array}\right\}\text{ total oxygen.. }15{\cdot}47$$

$$\begin{array}{ll}\text{Nitrogen}\ \ldots\ldots\ldots & 84{\cdot}53\\ \hline & 100{\cdot}00\end{array}$$

On comparing this with the composition of the common air (oxygen 20·96; carbonic acid 00·002; nitrogen 79·038) which had been introduced into the vessel, it is seen that 10·54 per cent. of oxygen has disappeared, while 5·05 per cent. of carbonic acid now exists, where only a trace of its presence could before be detected.

Similar results were obtained with defibrinated blood. In a case where defibrinated arterial blood from a calf, after complete saturation with oxygen, was kept in contact with an equal volume of air during twenty-four hours, and treated exactly as in the previous example, the gas on analysis yielded in 100 parts,—

$$\left.\begin{array}{ll}\text{Oxygen}\ldots\ldots\ldots & 11{\cdot}33\\ \text{Carbonic acid}\ \ldots. & 5{\cdot}96\end{array}\right\}\text{ total oxygen}=17{\cdot}29$$

$$\begin{array}{ll}\text{Nitrogen}\ \ldots\ldots\ldots & 82{\cdot}71\\ \hline & 100{\cdot}00\end{array}$$

showing in this case also that the air which had been imprisoned during twenty-four hours along with blood, no longer possessed its original composition, but that some of its constituents had been materially increased, while others had diminished in a manner no less marked.

It would appear from these examples that the blood had probably become oxidized in two ways; first, by giving off a quantity of car-

bon, and secondly by directly combining with oxygen. As to the portion of oxygen which has disappeared, and which is not accounted for by the carbonic acid evolved, it may have combined partly with another portion of carbon, to form a limited amount of carbonic acid, which by the law of absorption is retained in the blood; and partly with hydrogen or some other oxidable constituent of the blood, without yielding a gaseous product.

These two experiments it will be observed point to exactly the same conclusions, and together with a number of others, where the mode of procedure was similar, and which were attended with similar results, have satisfied the author as to the fallacy of Magnus's doctrine, "that the oxygen received during respiration into the blood is kept there merely by the law of mechanical absorption, and enters into no chemical combination with that liquid." Had this assertion been well-founded, such a change as has been seen to occur, in the composition of the air enclosed along with blood, saturated as the blood already was with oxygen, could not have happened.

After having ascertained that air underwent certain changes in composition during its contact with blood, it next became an object to discover by which of the constituents of the blood these changes were induced. With this view the author successively subjected the organic compounds of the blood separately to the action of air, by a similar process to that adopted in the case of the blood itself.

A certain quantity of fresh fibrin, moistened with water, was saturated with oxygen, placed in a receiver along with eight volumes of air, and kept during twenty-four hours at a temperature of from 20° to 25° cent. At the expiration of this time the gas on analysis was found to have the following composition:—

Oxygen..........	6·81	} total oxygen.. 17·98
Carbonic acid	11·17	
Nitrogen	82·02	
	100·00	

thus showing that fibrin takes up a certain quantity of oxygen, and gives off a stated amount of carbon combined with oxygen in form of carbonic acid gas.

The next experiments were made upon albumen, but as that substance could not be obtained in a pure, and at the same time un-

coagulated state from blood, the albumen of the hen's egg was employed, which possesses very similar characters. It was found that when a certain quantity of the white of the hen's egg was well saturated with oxygen, and afterwards kept in contact with an equal volume of air during a certain number of hours at a temperature of 36° cent., the gas on analysis gave in 100 parts,—

Oxygen	17·05
Carbonic acid	2·09
Nitrogen	80·86
	100·00

proving, in common with the experiments on the blood and on fibrin, that albumen also possesses the property of absorbing oxygen and disengaging carbonic acid.

Some comparative experiments were also made upon serum and upon blood-coagulum, in which it was found that the air confined along with the serum yielded on analysis—

Oxygen	16·74
Carbonic acid	2·30
Nitrogen	80·96
	100·00

while that confined with the coagulum contained—

Oxygen	8·57
Carbonic acid	7·29
Nitrogen	84·14
	100·00

It thus appears that the oxygen exerted a much more powerful action on the coagulum, which contained the fibrin and blood-corpuscles, than on the serum, which contained only albumen. The experiment thus corroborated the results previously obtained with pure fibrin and pure albumen. The pure fibrin was seen to produce a much greater change in the composition of the atmospheric air than the pure albumen from the hen's egg. The difference in the case of the coagulum and the serum was so much marked, that the author felt anxious to find out whence it proceeded; and under the impression that the hæmatin in the corpuscles might have mainly contributed to

the result (as other organic colouring matters possess the property of absorbing oxygen and giving off carbonic acid gas), he took a small quantity of pure blood-hæmatin prepared by Verdeil's process, and put it into a vessel along with 1000 volumes of ordinary air. After the air had been kept in contact with the hæmatin for some months, the gas was analysed and found to contain—

Oxygen	16·01
Carbonic acid	3·80
Nitrogen	80·19
	100·00

The pure colouring principle of the blood, therefore, by exposure to ordinary air, gives off carbonic acid gas, and becomes oxidized in two ways; first by a loss of carbon, and secondly by direct combination with oxygen. The author considers that this last result furnishes additional evidence of the correctness of an opinion he hazarded two years ago*, imputing to the colouring matters of the vegetable and animal economy a more important office in the function of respiration than they before had been considered to possess, and regarding their principal function in organized beings as the absorbing of oxygen and exhaling of carbonic acid—a view altogether irrespective of Liebig's well-known hypothesis, which assigns the above office to the iron of the blood-hæmatin.

The author concludes by expressing the hope that his experiments will be considered as at least serving to establish one important fact respecting which further evidence was wanted, namely, that the entire volume of the respired oxygen is not transmitted in an uncombined state (as Magnus believes) to the various organs and tissues of the body, but that a portion of it enters into chemical combination with some of the organic constituents of the blood.

* Verhand. Physik-Medizin. Gesellsch. zu Würzburg, Bd. v. 1854; and Erdmann's Journ. f. prakt. Chemie, Bd. lxiv. H. 5. 1855.

II. "Description of the Observatory of the Collegio Romano at Rome." By P. A. SECCHI. Communicated by GEORGE RENNIE, Esq., F.R.S.

Details are given of the construction of the different parts of the building, the arrangement of the instruments, &c., which, for the most part, could be rendered intelligible only with the aid of the plans and drawings with which the description is illustrated.

April 24, 1856.

The LORD WROTTESLEY, President, in the Chair.

The following communications were read :—

I. "Elements of a Mathematical Theory of Elasticity." By Professor WILLIAM THOMSON, F.R.S. Received April 16, 1856.

(Abstract.)

This paper consists of two parts: Part I. on Stresses and Strains; Part II. on the Mechanical Conditions of Relation between Stresses and Strains experienced by an Elastic Solid.

Part I.—The terms *Stress* and *Strain* are used in accordance with the valuable definitions by which they were first distinctively introduced into the Theory of Elasticity by Mr. Rankine*; with only this deviation; that instead of defining a stress as the reactive force exerted by an elastic body when in a condition of strain, the author of the present paper defines stress as "a definite external application of force to a body."

Various well-known theorems regarding the geometrical relations of the displacements among the parts of a body in a state of strain, and the geometrical representation of stresses and strains are enunciated, and briefly demonstrated, for the sake of convenience. A mode of expressing in absolute measure the magnitude of a stress or a strain, which the author believes to be new, is laid down nearly in the following terms. *The amount of work done by a stress applied to a body of unit volume, while acquiring a strain of the same type as the stress, is measured by the product of the magnitude of the stress into the magnitude of the strain.*

* "On Axes of Elasticity and Crystalline Forms," Proceedings, June 21, 1855.

When a stress and a strain are of the same type, they are said to be concurrent; or, if directly opposed, they are said to be negatively concurrent. When a stress and a strain are of any different types, the degree of their concurrence, or simply "their concurrence," is measured by *the work done by the stress applied to a body of unit volume acquiring the strain, divided by the product of the magnitude of the stress into the magnitude of the strain.* The measure of perfect concurrence is therefore $+1$, and that of perfect opposition -1. When work is neither spent nor gained in the application of a certain stress to a body while acquiring a certain strain, that stress and that strain, or any stresses or strains of the same types respectively, are said to be orthogonal to one another. The measure of their concurrence is zero.

A system of stress or strain coordinates involving symmetrically six independent variables, perfectly analogous to the system of triple coordinates for specifying the position of a point in space, is laid down. The concurrence of a stress or strain with six orthogonal types of reference being denoted by $l, m, n, \lambda, \mu, \nu$, it is demonstrated that

$$l^2+m^2+n^2+\lambda^2+\mu^2+\nu^2=1,$$

and it is proved that if $\cos\theta$ denote the mutual concurrence between two stress or strain types, whose concurrences with six orthogonal types of reference are respectively $(l, m, n, \lambda, \mu, \nu)$ and $(l', m', n', \lambda', \mu', \nu')$, we have

$$\cos\theta=ll'+mm'+nn'+\lambda\lambda'+\mu\mu'+\nu\nu'.$$

The treatment of the subject in the text of the paper is quite abstract, but along with it a series of examples are given, illustrating the statements by applications to familiar types of stresses and strains.

Part II. commences with an interpretation of the Differential Equation of the potential energy of Elasticity of a solid, in terms of the mode of specification of stresses and strains laid down in Part I. The Quadratic Function expressing the potential energy of an elastic solid when strained to an infinitely small amount, is next considered; and its simplest possible form, that of six squares with coefficients, is interpreted. Hence it is proved that *an infinite number of systems of six types of strains or stresses exist in any given elastic solid,*

such that if a strain of any one of those types be impressed on the body, the elastic reaction is balanced by a stress orthogonal to the five others of the same system.

It is next shown that there is necessarily one, and in general only one, such system of six types of strain for an elastic solid which are all mutually orthogonal; and the types belonging to this system are called the Six Principal Strain Types of the body.

The characteristic of a Principal Strain Type is, that the *stress* required to keep a body in a state of strain of such a type, *is of the same type as the strain.* The six Principal Elasticities of a body are the six coefficients by which strains of the six Principal Types must be multiplied to find the stress required to maintain them.

In conclusion, reasons are given for believing that natural crystals may exist for which there are six unequal Principal Elasticities, and consequently six different, and only six different, Principal Strain-types.

A corollary regarding the property which certain liquids and crystals possess of causing a rotation in the plane of polarization of light passing through them, and Faraday's optical property of transparent bodies under magnetic force, is inferred, and is more fully considered in a subsequent communication to the Royal Society.

II. "On the Construction of the Imperial Standard Pound, and its Copies of Platinum; and on the comparison of the Imperial Standard Pound with the Kilogramme des Archives." By W. H. Miller, M.A., F.R.S., Professor of Mineralogy in the University of Cambridge.—Part I. Received April 16, 1856.

(Abstract.)

The Commissioners appointed in 1838 to consider the steps to be taken for the restoration of the standards of weight and measure, to replace those which were destroyed by the burning of the Houses of Parliament, found provisions for the restoration of the lost standards prescribed to them by Sections 3 and 5 of the Act 5th George IV., whereby it is directed that, in case of the loss of the standards, the yard shall be restored by taking the length which shall bear a certain

relation to the length of the pendulum, vibrating seconds of mean time, in a vacuum, at the level of the sea; and that the pound shall be restored by taking the weight which bears a certain proportion to the weight of a cubic inch of water weighed in a certain manner. The Commissioners, however, in their Report dated December 21, 1841, decline to recommend the adoption of these provisions for the following reasons: "Since the passing of the said Act it has been ascertained that several elements of reduction of the pendulum experiment therein referred to are doubtful or erroneous. It is evident, therefore, that the course prescribed by the Act would not necessarily reproduce the length of the original yard. It appears also that the determination of the weight of a cubic inch of water is yet doubtful (the greatest difference between the best English, French, Austrian, Swedish and Russian determinations being about $\frac{1}{1200}$ of the whole weight, whereas the mere operation of weighing may be performed to the accuracy of $\frac{1}{1,000,000}$ of the whole weight). Several measures, however, exist, which were most carefully compared with the former standard yard; and several metallic weights exist which were most accurately compared with the former standard pound; and by the use of these the values of the original standards can be respectively restored without sensible error. And we are fully persuaded that, with reasonable precautions, it will always be possible to provide for the accurate restoration of standards by means of material copies which have been carefully compared with them, more securely than by experiments referring to natural constants."

At the end of the Travaux de la Commission pour fixer les Mesures et les Poids de l'Empire de Russie, Professor Kupffer has collected the results of observations made in France, England, Sweden, Austria and Russia for finding the weight of a given volume of water. The resulting values of the weight of an English cubic inch of water in a vacuum at 62° Fahr., expressed in doli, of which 22504·86 make a kilogramme, are as follows:—

French observations	368·365
English observations.............	368·542
Swedish observations	368·474
Austrian observations	368·237
Russian observations.............	368·361

Assuming the Russian observations to be the best, as they probably are, it will be seen that a troy pound deduced according to the method prescribed by the Act, would be 2·829 grains too heavy; while, if the Austrian observations had been accepted as the best, the troy pound would have been 4·707 grains too heavy. On the other hand, it was possible to recover the weight of the lost standard in air to within a fraction of 0·001 grain, by means of the troy pounds which had been compared with it, and could be easily brought together for recomparison. Seeing, then, that the error of one of these two methods of restoring the lost standard, is at least 2829 times as large as the error of the other method, the Committee could not hesitate to recommend the adoption of the latter.

A Committee was appointed by a Treasury Minute of June 20, 1843, to carry out the recommendations contained in the Report referred to above. The evidence for ascertaining the weight of the lost standard, placed at the service of this Committee, consisted of the following weights:—The brass troy pounds of the Exchequer Office; the brass troy pounds from the cities of London, Edinburgh and Dublin; the platinum troy pound and the two brass troy pounds then in the possession of Professor Schumacher; the platinum troy pound of the Royal Society; the troy pound used by the late Mr. Robinson of Devonshire Street, purchased by the Committee; four troy pounds made in 1758, two of which were formerly in the possession of Mr. Bingley of the Royal Mint, one the property of Messrs. Vandome and Titford, and one the property of the Bank of England.

The troy pounds of the Exchequer, and of the cities of London, Edinburgh and Dublin had been compared with the lost standard by Captain Kater in 1824. The three troy pounds in the custody of Professor Schumacher, and the troy pound of the Royal Society, were compared with the lost standard with extraordinary care in 1829 by the late Captain v. Nehus. The troy pounds bearing the date 1758 were constructed, along with the lost standard, by Mr. Harris, Assay Master of the Mint. These were referred to at the suggestion of Professor Schumacher, in the hope of arriving at a knowledge of the volume of the lost standard, which, unfortunately, had never been determined by weighing it in water. For, as long

as the volume of the lost standard remains unknown, the weight of the air displaced by it, and, consequently, its absolute weight, is uncertain within limits far exceeding the errors of weighing.

Let U denote the lost standard; Ex, L, Ed, D, RM the troy pounds of the Exchequer, the cities of London, Edinburgh, Dublin, and the Royal Mint, respectively; Sb, K two brass troy pounds, Sp a platinum troy pound, all in the custody of Professor Schumacher; RS the platinum troy pound of the Royal Society. Let Δ prefixed to the symbol by which any weight is designated denote the ratio of the density of the weight at the freezing-point to the maximum density of water; t the temperature of the air in degrees of Fahrenheit's scale; b the height of the mercury in the barometer in inches reduced to the freezing-point. The symbol $\triangleq$ placed between the symbols of two weights will be used to denote that they appear to be equal when weighed in air. The two weights in this case will not be equal unless their volumes are equal. When the weighings have been made in air of given density, or have been reduced to what they would have been in air of given density; or, when the volumes of the weights, the temperatures and pressures of the air being unknown, we are compelled to assume that their volumes are equal, the symbol $=$ may be substituted for $\triangleq$.

By the observations of Captain Kater (Philosophical Transactions, 1826),—

$$\begin{aligned}
\mathrm{Ex} &= \mathrm{U}+0{\cdot}0010 \\
\mathrm{L} &= \mathrm{U}+0{\cdot}0005 \\
\mathrm{Ed} &= \mathrm{U}+0{\cdot}0015 \\
\mathrm{D} &= \mathrm{U}+0{\cdot}0022 \\
\mathrm{RM} &= \mathrm{U}+0{\cdot}0021
\end{aligned}$$

By the observations of Captain v. Nehus in 1829—

No. of obs.		b.	t.
300	Sp $\triangleq$ U − 0·00857	29·722	65·62
140	RS $\triangleq$ U − 0·00205	29·806	65·73
60	Sb $\triangleq$ U − 0·01034	29·965	64·50
92	K $\triangleq$ U + 0·03389	29·646	65·09
16	RM $\triangleq$ U + 0·00887	29·679	65·91

$$10-\log\Delta\,\mathrm{Sp}=8{\cdot}67392,\quad 10-\log\Delta\,\mathrm{RS}=8{\cdot}67392,$$
$$10-\log\Delta\,\mathrm{Sb}=9{\cdot}08471,\quad 10-\log\Delta\,\mathrm{K}=9{\cdot}09724.$$

These weights were afterwards compared with each other with a balance of extreme delicacy procured from Mr. Barrow. In its construction it nearly resembled the balances of the late Mr. T. C. Robinson. The beam is made sufficiently strong to carry a kilogramme in each pan. Instead of having an index pointing downwards, as is usual in balances of this description, a thin slip of ivory is affixed to one end of the beam, a little more than half an inch long, divided into spaces of about 0·01 inch each. This scale is viewed through a compound microscope having a single horizontal wire in the focus of the eye-piece. A screen was interposed between the observer and the front of the balance-case, having a very small opening opposite to the eye-piece of the microscope.

In making a large number of comparisons, the weights compared are exposed to the risk of being injured by wear. In order to obviate this danger, two light pans were used of very nearly equal weight, each of which has a loop of wire forming an arch, the ends of which are attached to the pan at opposite extremities of a diameter of the pan. To the upper point of the loop of wire is affixed an iron hook. Each pan is suspended by a wire of suitable length bent into a hook at either end, from the ring attached to the agate plane resting on the knife-edge at either end of the balance.

Calling the weights of the pans X and Y, and the weights to be compared P and Q, P was placed in X and Q in Y, and $P+X$ compared with $Q+Y$ n times; then P was placed in Y and Q in X, and $P+Y$ compared n times with $Q+X$. The weights were thus exposed to the wear of two ordinary comparisons only in the course of $2n$ comparisons. The mean of the $2n$ comparisons gives the difference between P and Q unaffected by the very small, but unknown difference between X and Y. This contrivance was found to be especially useful when either of the weights to be compared consisted of several parts.

In using the method of double-weighing, the counterpoise was placed in the left-hand pan of the balance, and the detached pan X containing the weight P, and the detached pan Y containing the weight Q, were alternately suspended from the right-hand end of the beam, and the positions of equilibrium deduced in each case from the extreme positions of the beam at the beginning of each of three consecutive oscillations (usually twenty times). The weights were then

interchanged, and the pan Y containing the weight P, and the pan X containing the weight Q, suspended alternately from the right-hand end of the beam the same number of times.

In weighing by Gauss's method, in which the two weights to be compared as suspended from the right and left-hand ends of the beam respectively, and are then interchanged, it was desirable to be able to transfer the pans and the weights contained in them from one end of the beam to the other, without opening the doors of the balance-case, and thus avoid sudden changes of temperature of air in the balance-case, and consequent production of currents of air. In order to effect this, a slender brass tube 38 inches long was made to pass freely through two holes in the ends of the balance-case, which is nearly 23 inches long, near the top of the case and half-way between the balance and the front of the case. To the middle of the tube is attached a depending loop of wire. Suppose that by sliding the tube the loop is brought near to the right-hand end of the beam, and the pan with a weight in it transferred from the end of the beam to the wire loop by a brass rod having a hook at the end, which is inserted through a hole in the right-hand end of the balance-case. By sliding the tube in the opposite direction, the loop with the pan and weight suspended from it, is brought near to the left-hand end of the beam, to which it is transferred by a brass rod having a hook at the end, passing through a hole in the left-hand end of the balance-case. A similar tube halfway between the balance and the back of the case, serves to transfer the other pan and weight from one end of the beam to the other. In this manner any number of comparisons may be made without opening the balance-case, except in the middle of the series, for the purpose of changing the pans.

A sufficient number of preliminary comparisons of Sp, RS, Sb, K, Ex, L, Ed having been made in 1844, the results were reduced, when the material of one weight was platinum and that of the other brass, to what they would have been in air ($t=65{\cdot}66$, $b=29{\cdot}75$), or, of the mean density of the air during the comparisons of Sp and RS with U in 1829. Using U, Sp, RS, &c. to denote the apparent weights of U, RS, &c. in air ($t=65{\cdot}66$, $b=29{\cdot}75$), it was found that—

In 1829.	In 1844.
gr.	gr.
RS=Sp +0·0051	RS=Sp +0·0057
Sp =Sb +0·0022	Sp =Sb +0·0030
RS=Sb +0·0073	RS=Sb +0·0032
K =Sp +0·0420	K =Sp +0·0362
K =RS+0·0369	K =RS+0·0304
K =Sb +0·0442	K =Sb +0·0317

In the interval between 1829 and 1844, the difference between the two platinum troy pounds Sp and RS had undergone no very sensible relative change. If, as appears probable, Sp and RS have undergone no sensible absolute change, Sb has gained 0·0046 grain, and K has lost 0·0061 grain. On the same supposition it appears that—

In 1824.	In 1844.	Increase of	Interval in years.
gr.	gr.	gr.	
Ex −U=+0·0010	Ex −U=+0·0099	Ex 0·0089	20
L −U=+0·0005	L −U=+0·0151	L 0·0146	20
Ed −U=−0·0015	Ed −U=+0·0206	Ed 0·0221	20
D −U=+0·0022	D −U=+0·0248	D 0·0226	20
RM−U=+0·0021	(1829) RM−U=+0·0089	RM 0·0068	5

With the single exception of K, all the brass weights have become heavier since they were compared with U, in consequence probably of the oxidation of their surfaces, while U, which was made in 1758, was protected from further change by the coat of oxide already formed. One of these weights, Sb, appeared to have been protected by gilding, though imperfectly, since parts of its surface were slightly tarnished. Ex and L were brighter than Ed and D. K, though it had become lighter, was much tarnished. The discordances presented by the different weighings of K appear to have greatly perplexed both Professor Schumacher and Captain Kater, and were probably the cause of the numerous and accurate comparisons of the several troy pounds placed at the disposal of the Committee with the lost standard, on which alone depends the possibility of restoring it with sufficient accuracy. Previous to the comparison of K in 1844, a small fragment of wood, like a grain of coarse sawdust, was found adhering so firmly to its under surface, that it was detached with some difficulty. It appears probable that the changes of the weight

of K were caused by this bit of wood being weighed with it after the first comparison of K by Captain Kater, and by the gradual oxidation of the surface of K. The discrepancies presented by the weighings of the brass troy pounds at different times, due to the effect of oxidation or other causes, are so large, that I resolved, with the consent of the Astronomer Royal, to rest for the evidence of the weight of the lost standard entirely on the 300 comparisons of Sp and the 140 comparisons of RS with U.

If we consider the discordances presented by the weighings of the brass troy pounds simply as errors of observation, without paying any regard to their probable causes, the resulting value of U will not be very different from that given by the platinum troy pounds alone.

By the observations of 1824 and 1829,

	gr.	weight.
$U=Sp$	$+0{\cdot}0081$	30
$U=RS$	$+0{\cdot}0030$	14
$U=Sb$	$+0{\cdot}0103$	6
$U=K$	$-0{\cdot}0339$	9
$U=\frac{1}{4}(Ex+L+Ed+D)$	$-0{\cdot}0022$	6

By the observations of 1844,

		gr.
RS	$=Sp$	$+0{\cdot}0057$
Sb	$=Sp$	$+0{\cdot}0030$
K	$=Sp$	$+0{\cdot}0363$
$Ex+L+Ed+D$	$=2(Sb+K)$	$+0{\cdot}0260$

Whence, supposing the errors of weighing in 1844 to be insensible, compared with the discordances of the brass troy pounds,

	gr.	weight.
(1)	$U=Sp+0{\cdot}0081$	30
(2)	$U=Sp+0{\cdot}0087$	14
(3)	$U=Sp+0{\cdot}0133$	6
(4)	$U=Sp+0{\cdot}0024$	9
(5)	$U=Sp+0{\cdot}0261$	6

The mean of all the equations gives $U=Sp+0{\cdot}0096$ grain.

Excluding the last, which depends upon the weighings in 1824, $U=Sp+0{\cdot}0079$ grain.

Excluding all except the results of the comparisons of U with the two platinum troy pounds, U=Sp+0·0083 grain.

The temperatures were determined by means of three thermometers by Bunten, having centesimal scales etched upon the tube, and two thermometers having arbitrary scales traced upon the tubes with a diamond point. The zero-points of these were determined at distant intervals. They were often compared with each other, and, lastly, with an excellent standard thermometer constructed at Kew under the directions of Mr. Welsh, in order to form tables of the errors at any point of their scales, and to determine the position of their zeros at any given time. The barometer employed was a portable cistern barometer by Ernst of Paris, the scale of which was divided into millimetres. It was compared first with the standard barometer of the Paris Observatory, and afterwards with a standard barometer, having a tube of very large bore, belonging to the Taylor Library of Sidney Sussex College, Cambridge.

According to Ritter (Mémoires de la Société de Physique de Genève, t. iii. p. 361), the observations of Regnault show that in Paris, lat. 48° 50′ 14″, 60 metres above the mean level of the sea, a litre of dry atmospheric air, containing the average amount, 0·0004 of its volume, of carbonic acid, the density of which is 1·529 of that of atmospheric air at 0° Cent., under the pressure of 760 mm. of mercury at 0° Cent., weighs 1·2934963 gramme. If G be taken to denote the force of gravity at the mean level of the sea in lat. 45°, the force of gravity in lat. λ, at the mean level of the sea, $=G(1-0{\cdot}0025659\cos 2\lambda)$ (Baily, Mem. Ast. Soc. vol. vii. p. 94). The force of gravity in a given latitude at a place on the surface of the earth at the height z above the mean level of the sea $=\left\{1-\left(2-\frac{3}{2}\frac{\rho'}{\rho}\right)\frac{z}{r}\right\}\times$ force of gravity at the level of the sea in the same latitude, where r is the radius of the earth, ρ its mean density, and ρ' the density of that part of the earth which is above the mean level of the sea (Poisson, Traité de Mécanique, t. ii. p. 629).

According to Regnault, the expansion of air under constant pressure from 0° to 100 Cent., is 0·36706 of its volume at 0° Cent.; also at 50° Cent., the mercurial thermometer is about 0°·2 in advance of the air thermometer (Mémoires de l'Institut, t. xxi. p. 91. p. 238, Annales de Chimie, 3 série, t. v. p. 99). Hence, density air at

0° : density air at $t = 1 + 0{\cdot}003656t$. The density of the vapour of water is 0·622 of that of air. Hence, if t be the temperature of the air in centesimal degrees, b its barometric pressure, v the pressure of vapour, both in millimetres of mercury at 0° Cent., the weight in grammes of a litre of air at a place on the surface of the earth at a height z above the mean level of the sea in lat. λ, will be

$$\frac{1{\cdot}2930693}{1+0{\cdot}003656t}\,\frac{b-0{\cdot}378v}{760}\left(1-1{\cdot}32\frac{z}{r}\right)(1-0{\cdot}0025659\cos 2\lambda).$$

Regnault finds that in rooms not heated artificially, the pressure of vapour is two-thirds of the maximum pressure corresponding to the temperature (Memorie della Società Italiana della Scienze in Modena, t. xxv. p. 1).

The weight of air used in reducing the weighings was calculated from the above expression.

The mean rate of expansion of brass, for 1° Cent., from 0° Cent. to 100° Cent., usually assumed 0·0000187 of its length at 0° Cent., is considerably larger than the rate of expansion at ordinary atmospheric temperatures, according to the observations of Mr. Sheepshanks, who found that at about 17° Cent. the coefficient of the linear expansion of brass $=0{\cdot}00001722$ for 1° Cent. This value of the expansion has been accordingly adopted.

The linear expansion of platinum is assumed to be 0·00000900 for 1° Cent., as given by Schumacher in his first table (Phil. Trans. 1836). The expansion of water is calculated from a mean of the experiments of Despretz, I. Pierre and Kopp, corrected for the error of the assumed expansion of mercury by Regnault's observations, and assuming the temperature of maximum density to be 3°·945 Cent., in accordance with the result obtained by Messrs. Playfair and Joule. The logarithms of the expansion to 7 places considered as integers, are given with sufficient accuracy, between 4° Cent. and 25° Cent., by $32{\cdot}72(t-3{\cdot}945)^2-0{\cdot}215(t-3{\cdot}945)^3$.

Though it appears that only two of the nine weights with which U was compared in 1826 and 1829 are in a state of unexceptionable preservation, and that the number of trustworthy comparisons is reduced from 669 to 440, these are amply sufficient for the purpose of ascertaining the weight of U in air ($t=65°{\cdot}66$ Fahr., $b=29{\cdot}75$ inches). But in order to find the absolute weight of U, or indeed

its apparent weight in air of a density different from that which it has when $t=65^{\circ}\cdot66$ Fahr., $b=29\cdot75$ inches, a knowledge of the volume of the lost standard is requisite. An indirect way of arriving at it was suggested by Professor Schumacher, by an examination of certain Parliamentary Reports, presented May 26, 1758, April 11, 1759, March 2, 1824. It appears from the first of these, that Mr. Harris, then Assay Master of the Mint, presented to the Committee three troy pounds made under his direction, one of which was the lost Imperial standard; and from the third, that one of the two remaining pounds came into the possession of Mr. Vandome, and the second into the possession of Mr. Bingley of the Mint. Professor Schumacher then observes that we can still either determine, with the highest degree of probability, the density of the lost Imperial standard, or know with certainty that all hope to arrive at this knowledge is lost. It will be only requisite to ascertain with the greatest care the densities of both pounds, the one in the possession of Mr. Bingley, the other in the possession of Mr. Vandome. If the density of both is found the *same*, we might from that circumstance draw the highly probable conclusion, that the three single pounds of Mr. Harris, according to my hypothesis, were really made of the same identical metal; and the density of the two remaining pounds might with safety be considered as that of the lost standard. If, on the contrary, the two remaining pounds prove to be of *different* densities, the hypothesis that all three were made of the same metal is evidently erroneous; and nothing can be inferred from the density of either of the two remaining.

Mr. Vandome readily consented to allow his troy pound to be experimented upon by the Committee. Denoting this weight by the letter V, by weighing in air and in water it was found that $\Delta V=8\cdot15084$, and that it was about $0\cdot309$ grain lighter than U.

Mr. Bingley had in his possession two troy pounds, both dated 1758. One of these, O, said to be the original weight from which the standard was made for the House of Commons in 1758, has since been purchased by the Committee; the other, M, has been presented to the Mint by Mr. Bingley. As Mr. Bingley was unwilling to permit either of these weights to be weighed in water, Messrs. Troughton and Simms were commissioned to construct an instrument on the principle of the Stereometer invented by M. Say for the

purpose of finding the density of gunpowder (Ann. de Chimie, 1797, t. xxiii. p. 1), but with some improvements which I had described in the Philosophical Magazine for July and December, 1834, vol. v. p. 203. Let v prefixed to the symbol of any weight denote the volume of that weight at 0° Cent., the unit of volume being the volume of a grain of water at its maximum density. Then, by means of the Stereometer, it was found that $v\mathrm{V}-v\mathrm{O}=22\cdot68$, $v\mathrm{V}-v\mathrm{M}=17\cdot38$. These differences show that the volume of lost standard cannot be inferred with any high degree of probability from a comparison of the remaining pounds. The only resource now remaining was indicated by Professor Schumacher's remarks on the figure of the lost standard:—"As soon as the Imperial standard troy pound was brought to Somerset House, Captain Nehus's first care was to make an accurate drawing of its shape and marks, measuring all its dimensions with the greatest care. The annexed drawing represents this pound in its actual dimensions; and is now, since the original has been destroyed by the calamitous fire that consumed the two Houses of Parliament in 1834, the only thing remaining which can preserve an idea of it." By a comparison of the figure of U in the Philosophical Transactions for 1836, with a profile of V traced mechanically, the axis and the extreme diameter of the knob and cylindrical portion of U, appeared to be a very little greater than the corresponding dimensions of V. On comparing the profiles of U and V, it did not seem possible to suppose that the volume of U was less than that of V. But the volume of O, as well as that of M, being less than that of V, it appeared that of the three weights V, O, M, V approximated most nearly to U in volume. As the existing data were utterly insufficient to determine how much, if at all, U exceeded V in volume, it appeared safest to assume the volumes of U and V to have been equal. This course was also recommended by Professor Schumacher.

It was afterwards found that O was 0·144 grain lighter than U, $\Delta\mathrm{O}=8\cdot4004$; and that M was 0·047 grain lighter than U, $\Delta\mathrm{M}=8\cdot3491$.

In a letter from William Miller, Esq., of the Bank of England, dated August 22, 1855, I was apprised of the existence of a fourth troy pound of 1758. This weight was 0·249 grain heavier than U; its density $=8\cdot3175$.

If U, the lost standard, be supposed to have the same density as V, the comparisons of Sp and RS with U by Captain v. Nehus in 1829, give,—

$$Sp = U - 0{\cdot}52959$$
$$RS = U - 0{\cdot}52444.$$

The Commissioners for the Restoration of the Standards of Weight and Measure, in their Report, dated December 21, 1841, recommended that the avoirdupois pound of 7000 grains be adopted instead of the troy pound of 5760 grains, as the New Parliamentary Standard of Weight, and that the new standard and four copies of it be constructed of platinum.

In accordance with this recommendation, five weights were made by Mr. Barrow, a little in excess of 7000 grains, of platinum prepared by Messrs. Johnson and Cock. The form of these pounds is that of a cylinder, nearly 1·32 inch in height and 1·15 inch in diameter, with a groove round it, the middle of which is about 0·34 inch below the top of the cylinder, for insertion of the prongs of a forked lifter of ivory. They are marked PS 1844 1 lb.; PC No. 1 1844 1 lb.; P CNo. 2 1844 1 lb.; PC No. 3 1844 1 lb.; PC No. 4 1844 1 lb., respectively.

The weights of 7000 grains might have been derived from that of 5760 grains, by the use of either a decimal or a binary system of weights. In either case, however, the number of weights to be compared with one another and with the weights of 7000 and 5760 grains would have been large, and the errors of their comparisons among themselves might, by their accumulation, sensibly affect the resulting weight of 7000 grains. Also, the repeated comparison of weights made up of the sum of several others, was a very troublesome process, previous to the use of the detached pans, already described, which had not been thought of when the weights were ordered.

These two evils were in a great measure avoided by the use of a platinum weight T of about 5760 grains, or more correctly very nearly equal to Sp or RS, and of the following auxiliary weights, also of platinum, and all constructed by Mr. Barrow: A, B, C, D each of 1240 grains; F of 800 grains; G of 440 grains; H of 360 grains; K, L, M, N each of 80 grains; R, S each of 40 grains, nearly. The numbers of the weights of each denomination, and their values, are given by the quotients and divisors obtained in the conversion of

$\frac{7000}{5760}$ into a continued fraction. The errors of these weights are found by the following comparisons:—Sp and RS with T; T with A+B+C+D+F; each of the weights A, B, C, D with F+G; F with G+H; G with each of the weights H+K, H+L, H+M, H+N; H with K+L+M+N+R and K+L+M+N+S; each of the weights K, L, M, N with R+S.

Sp and RS, instead of being true troy pounds, and, consequently, equal to U in a vacuum, had been adjusted so as to appear nearly as heavy as U when weighed in air of ordinary density, and are therefore lighter than U by about 0·53 grain, the weight of the air contained in the space equal to the difference between the volume of U and that of Sp or RS. A space equal to the difference between the volume of 7000 grains of metal of the assumed density of U, and 7000 grains of platinum, contains about 0·645 grain of air. Calling this Q, PS may be compared with each of the weights T+A+Q, T+B+Q, T+C+Q, T+D+Q. In order to determine Q with the greatest precision, Mr. Barrow supplied ten weights Q of about 0·645 grain each, so accurately adjusted that no appreciable difference could be detected between them; a weight V of 6·451 grains, and a weight W of 12·901 grains, all of platinum. Then Y and Z being two platinum weights of 20 grains each, the following comparisons became possible:—each of the weights R and S with Y+Z; each of the weights Y and Z with W+V+ each of the weights Q in turn; W with V+ sum of ten weights Q; V with the sum of the ten weights Q. In comparing PS with each of the weights T+A+Q, T+B+Q, T+C+Q, T+D+Q, the weight Q was changed at the end of every four comparisons, and thus each of the ten weights Q used in turn in a series of forty comparisons.

By numerous weighings in air and in water the densities of the several weights were found to be as follows:—

T	21·1661
PS	21·1572
PC No. 1 ..	21·1671
PC No. 2 ..	21·1640
PC No. 3 ..	21·1615
PC No. 4 ..	21·1556

By 286 comparisons of T with Sp and 122 comparisons of T with

RS, assuming the density of U to have been the same as that of V, T=5759·47141 grains, of which U contained 5760. By numerous comparisons of the auxiliary weights with each other and with T, A=1239·88621, B=1239·88604, C=1239·88596, D=1239·88579, Q=0·64509.

By 80 comparisons of PS with each of the weights T+Q+A, T+Q+B, T+Q+C, and 100 of PS with T+Q+D,

	gr.	t.	b.
PS ≏	T+Q+A −0·002936	19·47	758·38
PS ≏	T+Q+B −0·001731	19·19	759·31
PS ≏	T+Q+C −0·001621	18·83	754·38
PS ≏	T+Q+D −0·000774	19·63	764·43
Mean PS ≏	T+Q+¼(A+B+C+D)−0·00177	19·28	759·12

whence, supposing U to have the same density as V,

PS=7000·00090 grains, of which U contained 5760.

Results of comparisons of PC No. 1, PC No. 2, PC No. 3, PC No. 4 with PS:—

gr.	No. of Comparisons.
PC No. 1=PS+0·00051	200
PC No. 2=PS−0·00089	216
PC No. 3=PS−0·00178	204
PC No. 4=PS−0·00316	204

The weights Sp, Sb, K were returned to Professor Schumacher accompanied by a weight V such that, by a mean of 200 comparisons, Sp+V≏PS−0·00071 grain in air (t=13·1, b=759·09).

By the good offices of M. Arago, permission was obtained from the French Government to compare the new English weights with the standard kilogramme of platinum, known as the *kilogramme des Archives*, and which will be denoted by the letter 𝔄. The comparison was made by two perfectly independent methods. In one of these 𝔄 was compared sixty times with PC No. 1 + PC No. 2 + auxiliary weight B+ a platinum weight V of nearly 192·436 grains. In the other, 𝔄 was first compared 200 times with the platinum kilogramme 𝔈, purchased for the British Government. 𝔈 was afterwards compared with PS+ each of the four platinum copies of the

pound in succession, together with a platinum weight of about 1432·324 grains, the weight of which was found with great precision by a process to be described presently.

$\mathfrak{A}$ had never been weighed in water. By observations made with the stereometer, it was found that at 0° C. the volume of $\mathfrak{A}$ exceeded that of $\mathfrak{E}$ by a quantity equal to the volume of 21·119 grains of water at its maximum density. By weighing $\mathfrak{E}$ in air and in water, it was found that $\Delta\mathfrak{E}=20{\cdot}54877$. Some time after these observations were made, the Committee received from Professor Schumacher some observations of his own in manuscript, and a copy of Professor Steinheil's paper, entiled 'Das Bergkrystall-kilogramm,' from the fourth volume of the Transactions of the Bavarian Academy of Sciences, containing the determination of the volume of $\mathfrak{A}$, by comparing its linear dimensions with those of a platinum kilogramme of his own $\mathfrak{S}$, the density of which had been found by weighing it in air and in water. The two weights being cylinders, and the linear dimensions measured with an extremely delicate instrument constructed by Gambey, this kind of observation admitted of being made with great accuracy. The resulting difference between the volume of $\mathfrak{A}$ and that of $\mathfrak{E}$, was found to be equal to the volume of 20·933 grains of water at its maximum density. On account of the large number of observations, and the extreme care with which they were made, this value of the volume $\mathfrak{A}$ — volume $\mathfrak{E}$ is to be preferred to that which was obtained by the stereometer, and has accordingly been used in reducing the observations for comparing the weights of $\mathfrak{A}$ and $\mathfrak{E}$.

$\mathfrak{E}$ was compared with PS by the method which had proved so satisfactory in deducing the avoirdupois pound from the troy pound. Let I, K, L, M, N denote PS and its four platinum copies, A, B, Γ, Δ, platinum weights of about 1432·322 grains each, Z a weight of about 1270·708 grains, Θ a weight of 161·629 grains, made up of weights the values of which had been carefully determined. $\mathfrak{E}$ was compared with each of the weights $I+K+A$, $I+L+B$, $I+M+\Gamma$, $I+N+\Delta$, each of the lbs. K, L, M, N having been previously compared with I; I with $A+B+\Gamma+\Delta+Z$; each of the weights A, B, Γ, Δ with $Z+\Theta$. In this manner it was found that the kilogramme des Archives weighed 15432·34874 grains, of which the new Imperial Standard pound contains 7000, or kilogramme $=2{\cdot}20462125$ lb. This is pro-

bably the best determination of the weight of 𝔄 in terms of the English standard of weight.

The value of 𝔄, as deduced from the direct comparison of 𝔄 with K+L+B+V, is subject to some uncertainty, arising from the circumstance that the platinum, of which A, B, C, F were made, had been very badly prepared and contained cavities filled with some hygroscopic substance which rendered the weight of B slightly variable, according to the greater or less amount of moisture present in the atmosphere. According to these observations, the kilogramme des Archives =15432·34816 grains.

By the observations of Schumacher and Steinheil on the ratio of the weight of 𝔄 to that of Sp, subject to an uncertainty of 0·00139 grain, on account of an error of the press, and the comparison of Sp with PS, the weight of 𝔄 is either 15432·34873 or 15432·35012 grains, of which PS contains 7000.

The French standard of commercial weight is a brass kilogramme 𝔏, known as the *kilogramme type laiton*. It is deposited at the Ministère de l'Intérieure. According to a comparison of 𝔏 with 𝔄, the result of which is published in the 25th volume of the Modena Transactions, the apparent weight of 𝔏, when weighed in air at Somerset House, the mercury in the barometer, reduced to the freezing-point, standing at 29·75 inches, and the thermometer at 65·66 F. (b=755·64 mm., t=18·7 C.), is 15432·344 grains, of which the English commercial standard contains 7000.

The Society then adjourned to Thursday, May 8.

May 8, 1856.

The LORD WROTTESLEY, President, in the Chair.

In accordance with the Statutes, the Secretary read the following list of Candidates recommended by the Council for election into the Society:—

John Hutton Balfour, M.D.
Edward W. Binney, Esq.
Sir John Bowring.
Sir John Fox Burgoyne, Bart.
Philip Henry Gosse, Esq.
Robert Harkness, Esq.
Cæsar Henry Hawkins, Esq.
Manuel John Johnson, Esq.
John Carrick Moore, Esq.
Henry Minchin Noad, Esq.
Edmund Potter, Esq.
Rev. T. Romney Robinson, D.D.
Henry Hyde Salter, M.D.
Archibald Smith, Esq.
Capt. Thomas A. B. Spratt, R.N.

The following communications were read:—

I. "On various Phenomena of Refraction through Semi-lenses or Prisms, producing anomalies in the illusion of Stereoscopic Images." By A. Claudet, Esq., F.R.S. Received April 22, 1856.

(Abstract.)

The author having observed that photographic pictures representing flat surfaces, when examined in the refracting stereoscope, have the appearance of concavity, has endeavoured to discover the cause of that phenomenon, and to explain it.

In order to ascertain if this peculiar effect was attributable to some imperfection in the lenses of the camera obscura which had produced the photographic pictures, or to a property of the stereoscope itself, he began to test the stereoscope without photographic

images. For this experiment he placed under each tube of the stereoscope a diagram composed of vertical and horizontal lines crossing each other.

The two diagrams, perfectly identical when seen in the stereoscope, coalesced and formed only one figure; but although each diagram, when seen separately by its corresponding eye, appeared perfectly flat, still the coalescing image of the two presented a surface conspicuously concave; consequently there was no doubt that the same illusion observed in photographic pictures was due only to the effect of the stereoscope. This experiment was decisive, and it remained to discover how the illusion was produced. The investigations showed that the phenomenon, which is a defect detrimental to the beauty and correctness of the stereoscopic representations and unavoidable in the refracting stereoscope, is a plain illustration of the cause of relief and distance, and yield the clearest explanation of the stereoscopic illusion,—proving that it is founded on the true principles of natural binocular vision.

When we look through a prism placed near the eye at a straight line, the refracting edge of the prism being parallel with the straight line, that line is refracted laterally and appears bent, with its concave side turned to the thin edge of the prism. The two tubes of the stereoscope being supplied with semi-lenses acting as prisms, each lens bends all vertical straight lines, and the concave sides of these lines are turned towards the thin edges of the lenses, and consequently towards each other. When we examine in the stereoscope two curved lines having their concave sides turned towards each other, the result of the coalescing of these two lines is a concave line, the extremities appearing nearer and the centre further. If the convex sides are turned towards each other, the result of the coalescence is a convex line, the extremities appearing further and the centre nearer. By the same reason, if straight lines are bent by the prismatic refraction of the two semi-lenses, as the bending is effected so that the concave sides are turned towards each other, the result is by coalescence a concave line. The two photographic images will have all their vertical lines bent in the same manner, and the stereoscope will give the illusion of a picture represented on a concave surface.

When we look at natural objects, the optical axes have to con-

verge more for the nearest than for the furthest, in order to obtain a single vision by bringing the same object on the centre of each retina; therefore by habit we judge of the distances by the angle formed by the optical axes required to obtain a single vision. Again, while we look at one object, while other objects in the same line are situated before and behind that object, we have the sensation of their double images on the two retinæ. The double images of nearer objects are situated in the following order: one on the right of the centre of the right retina, and the other on the left of the centre of the left retina; and the double images of further objects, one on the left of the right retina, and the other on the right of the left retina.

In looking at the two pictures in the stereoscope, we have to converge the optical axes on one point which is beyond the plane of the pictures, so that two of their correspondent or similar points appear respectively on the two lines forming the angle of convergence of the optical axes, and each of these points is represented on the centre of one retina. As the two corresponding points of the two pictures are laterally nearer each other for the first plane and more distant for the receding plane, it follows that the optical axes have to converge beyond the plane of the pictures on a nearer point for the first, and on a further point for the last. Therefore, the angle of convergence by which similar points of the two pictures appear on each axis and consequently fall on the centre of each retina, conveys the sensation of their respective distances; more convergence indicates less distance, and less convergence more distance. All the other corresponding points of the two pictures which are not on the optical axes or on similar points of the two retinæ, form double images; and when we look at one point, all the points of nearer and further planes appear double in the same order on the two retinæ, as when we look in like manner at natural objects; and the situation of double images seen through the stereoscope indicates the distances of the objects they represent, according as one is on the right of the right retina and the other on the left of the left retina, or one on the left of the right retina and the other on the right of the left retina.

This being explained, it is easy to understand what will be the stereoscopic result of vertical lines represented as curved, and having their concavities turned toward each other. The two correspondent points of the top and bottom of the two concave lines, being nearer

each other, will require more convergence than the two corresponding points of the centres of the concave lines, and will appear nearer, whilst the two points of the centre requiring less convergence will appear further; the intermediate points from the centres to the extremities of the two bent lines will appear gradually less distant, therefore the coalescence of the two lines bent laterally will produce the illusion of a single line conspicuously concave, in a vertical plane at right angles with the plane of the two separate lines.

Having demonstrated that the semi-lenses of the stereoscope, like prisms, bend laterally all the vertical lines of which the photograph pictures are composed, and that these lines in the two pictures present their concavity to each other, it is evident that the coalescence of the two images must give the illusion of a concave image.

The phenomenon of the lateral curvature given to vertical lines by the refraction of a prism, which vertical lines, when examined with two prisms, one for each eye, appear by coalescence as one line concave in a vertical plane at right angles with the plane of the two separate bent lines, can be curiously illustrated by the following experiment:—

If, holding in each hand one prism, the two prisms having their thin edges towards each other, we look at the window from the opposite end of the room, we see first two windows with their vertical lines bent in contrary directions; but by inclining gradually the optical axes, we can converge them until the two images coalesce, and we see only one window; as soon as they coincide, the lateral curvature of the vertical lines ceases, and they are bent projectively from back to front: we have then the illusion of a window concave towards the room, such as it would appear reflected by a concave mirror.

There is another phenomenon which can be noticed when looking at photographic pictures in the stereoscope; sometimes the picture appears to project out and sometimes to recede from its mountings. The first effect lessens the illusion, and the second renders it more effectual; therefore it is desirable to inquire how we can avoid the one and ensure the other.

We know that the distance of objects is in an inverse ratio with the angle of convergence required to see them single; also that with symmetrical figures or photographic pictures, when the horizontal or lateral distances of the several corresponding points is different, the

points less separated will appear nearer, and the more separated will appear further.

Suppose the two correspondent vertical lines of the openings or frames of the pictures be more distant than the two correspondent points of the furthest plane of the pictures themselves, then the openings or frames will appear behind the pictures; and suppose the correspondent vertical lines of the openings be less distant than the two correspondent points of the nearest plane of the pictures themselves, then the openings or frames will appear before the picture.

Therefore, when we wish to have the picture appearing behind the openings or their mountings, we have only to take care that the correspondent vertical lines of the mountings should be laterally less distant than the two correspondent points of the first plane of the picture. This can be easily done by taking the measure of the two correspondent points of the first plane by means of a pair of compasses, and tracing the two pairs of correspondent vertical lines bounding the openings, after having slightly reduced the angle of the compasses.

A very simple experiment may show the cause of the illusion of concavity of flat surfaces when examined through semi-lenses, and further prove that semi-lenses may give alternately the illusion of concavity and convexity according to the position of their thin edges; of concavity when their edges are towards each other, and of convexity when they are placed contrariwise. For this experiment we have only to employ a pair of those spectacles mounted with a spring whereby they are held on the nose.

When we read, holding such spectacles with both hands, we may by the elasticity of the spring adjust the two lenses so that the pupils of the eyes can coincide, first, with the two nearest edges, secondly, with the two centres, and thirdly, with the two furthest edges of the lenses.

In the first case, the page of the book will appear concave, because the pupils will look through the thin edges of the lenses which bend the vertical lines with their concave sides turned towards each other; in the second, the page will appear flat, because the pupils will look through the centres of the lenses which show the vertical lines perfectly straight; and in the third case, the page of the book will appear convex, because the pupils will look through the thin edges of

the lenses which bend the vertical lines with their convex sides turned towards each other.

These considerations have led the author to construct a stereoscope which presents flat surfaces perfectly flat. This new stereoscope has two entire lenses instead of two semi-lenses, and the eyes look through the centre of such lenses. The images not being laterally refracted, as in the semi-lenticular stereoscope, their coalescence requires a certain effort of divergence, or to squinting outwards, which a little practice will enable us to perform easily. Persons capable of using this kind of stereoscope will see a picture whose surface is perfectly flat with all the illusion of relief and distance.

All lenses being more or less subject to the defect of bending straight lines when refracted by all the various points of their surface but the centre, and in a greater degree as those points are nearer the edges, it results that when images are produced in the camera obscura by the various points of the whole aperture, they will be bent in various contrary directions, and a certain confusion must arise injurious to the delicacy and correctness of the whole compound image. This may be proved by the following experiments:—If we take the image of a window by a small aperture placed on the right edge of a lens, say of 3 inches aperture, and another image of the same window, by placing the aperture on the left, taking care to shift the camera so that the two apertures will be exactly on the same line, we shall have two images of the same window apparently identical; but in placing these two images side by side in the central lens-stereoscope above described, first the image of the left side aperture on the right, and that of the right side aperture on the left, secondly the images *vice versâ*, we shall see in the first case a concave window, and in the second a convex window. But in examining the two images in the semi-lenticular stereoscope, we shall see in one case a concave window, and in the other a perfectly flat window, because in the first case the stereoscope will have increased the bending of the vertical lines of the two images, and in the second the stereoscope will have corrected the bending.

This fact naturally suggests the possibility of correcting the defect of the refracting stereoscope; for if the images of the camera were taken by semi-lenses, the bend resulting from this mode of operating might be corrected by the bend of the stereoscope, care being taken

to turn the thin edge of the semi-lenses of the two cameras in the direction which will produce a bending contrary to that of the semi-lenses of the stereoscope.

Having shown how the lateral proportional distances of any two correspondent points of the two stereoscopic pictures are the indices of their perspective distances, if we were, while looking in the stereoscope, to produce a change in those proportional lateral distances by sliding horizontally in a contrary direction, two pairs of superposed glass photographic pictures, the objects would appear to move, not in the horizontal lateral direction of that change which they naturally have, but in a straight line forward and backward, as if the object was approaching or receding.

But the most curious effect of that motion would be, that the objects would appear increasing in size while they were receding, and diminishing while approaching, which we know is contrary to the rule of perspective. This is another illusion entirely physiological, and the cause of which may be thus explained; while the object appears moving forward and backward it remains always the same size, but as we expect when it moves forward that it should increase in size, and when it moves backward that it should decrease, and as it does not, we feel that it is diminishing when approaching and increasing when receding.

II. "A Memoir upon Caustics." By Arthur Cayley, Esq., F.R.S. Received May 1, 1856.

(Abstract.)

The principal object of this memoir, which contains little or nothing that can be considered new in principle, is to collect together the principal results relating to caustics *in plano*, the reflecting or refracting curve being a right line or a circle, and to discuss with more care than appears to have been hitherto bestowed upon the subject, some of the more remarkable cases. The memoir contains in particular researches relating to the caustic by refraction of a circle for parallel rays, the caustic by reflexion of a circle for rays proceeding

from a point, and the caustic by refraction of a circle for rays proceeding from a point; the result in the last case is not worked out, but it is shown how the equation in rectangular coordinates is to be obtained by equating to zero the discriminant of a rational and integral function of the sixth degree. The memoir treats also of the secondary caustic or orthogonal trajectory of the reflected or refracted rays in the general case of a reflecting or refracting circle and rays proceeding from a point; the curve in question, or rather *a* secondary caustic, is, as is well known, the Oval of Descartes or 'Cartesian:' the equation is discussed by a method which gives rise to some forms of the curve which appear to have escaped the notice of geometers. By considering the caustic as the evolute of the secondary caustic, it is shown that the caustic in the general case of a reflecting or refracting circle and rays proceeding from a point is a curve of the sixth class only. The concluding part of the memoir treats of the curve which, when the incident rays are parallel, must be taken for the secondary caustic in the place of the Cartesian, which, for the particular case in question, passes off to infinity. In the course of the memoir, the author reproduces a theorem first given, he believes, by himself in the Philosophical Magazine, viz. that there are six different systems of a radiant point and refracting circle which give rise to identically the same caustic. The memoir is divided into sections, each of which is to a considerable extent intelligible by itself, and the subject of each section is for the most part explained by the introductory paragraph or paragraphs.

III. "On the Figure, Dimensions, and Mean Specific Gravity of the Earth, as derived from the Ordnance Trigonometrical Survey of Great Britain and Ireland." Communicated by Lieut-Colonel James, R.E., F.R.S., &c., Superintendent of the Ordnance Survey. Received April 30, 1856.

(Abstract.)

The Trigonometrical Survey of the United Kingdom commenced in the year 1784, under the immediate auspices of the Royal Society; the first base was traced by General Roy on the 16th of April of

that year, on Hounslow Heath, in presence of Sir Joseph Banks, then President of the Society, and some of its most distinguished Fellows.

The principal object which the Government had then in view, was the connexion of the Observatories of Paris and Greenwich by means of a triangulation, for the purpose of determining the difference of longitude between the two observatories.

A detailed account of the operations then carried on is given in the first volume of the 'Trigonometrical Survey,' which is a revised account of that which was first published in the 'Philosophical Transactions' for 1785 and three following years.

At the time when these operations were in progress, the Survey of several counties in the south-east of England, including Kent, Sussex, Surrey, and Hampshire, was also in progress, under the direction of the Master-General of the Ordnance, for the purpose of making military maps of the most important parts of the kingdom in a military point of view; and it was then decided to make the triangulation which extended from Hounslow to Dover the basis of a triangulation for these surveys.

It is extremely to be regretted that a more enlarged view of the subject had not then been taken, and a proper geometrical projection made for the map of the whole kingdom. As it is, the south-eastern counties were first drawn and published in reference to the meridian of Greenwich, then Devonshire in reference to the meridian of Butterton in that county, and thirdly the northern counties, in reference to the meridian of Delamere in Cheshire; but there is a large intermediate space, the maps of which are made of various sizes to accommodate them to the convergence of the meridian.

In 1799 the Royal Society gave further proof of the interest it took in the progress of the Survey, by lending to the Ordnance its great 3-foot Theodolite, made by Ramsden, for the purpose of expediting the work of the Survey; and although this instrument has been in almost constant use for the last sixty-seven years, during which time it has been placed on the highest church towers and the loftiest mountains in the kingdom, from the Shetlands to the Scilly Islands, it is at this day in perfect working order, and probably one of the very best instruments that was ever made.

The great Trigonometrical operations of the Survey have been

carried on under so many officers, from the time of their commencement under General Roy down to the present time, that it would be quite impossible, in this short notice, to mention more than the names of several Superintendents who have succeeded General Roy, viz. Colonel Williams, Major-General Mudge, Major-General Colby, and Colonel Hall; but in justice to the highly meritorious body of non-commissioned officers of the Corps of Royal Sappers and Miners, it should be stated, that whilst in the early part of the Survey the most important and delicate observations were entrusted solely to the commissioned officers, these duties have of late years been performed by the non-commissioned officers with the greatest skill and accuracy.

The computations connected with the corrections of the observed angles, to make the whole triangulation as nearly as possible perfectly consistent, have been most voluminous, and have been made under the direction of Lieut.-Colonel Yolland, Captain Cameron, and Captain Alexander R. Clarke; but Col. James gladly avails himself of this opportunity to acknowledge the great and important assistance and advice which, both as regards the instruments and the calculations, have at all times been received from the Astronomer Royal.

The triangulation, by the methods which will be explained, is now made consistent in every part, so that any side of any triangle being taken as a base, the same distance will be reproduced when it is computed through any portion or the whole series of triangles; and when the five measured bases relied on are incorporated in this triangulation, the greatest difference between their measured and computed lengths is not as much as 3 inches, and yet some of the bases are upwards of 400 miles apart.

Several bases of from five to seven miles long have been measured, but those upon which the chief reliance has been placed are the Lough Foyle and Salisbury Plain bases which were measured with General Colby's compensation bars. The difference between the measured and computed length of the one base from the other through the triangulation is 0·4178 ft., or about 5 inches.

This difference has been divided in proportion to the square root of the lengths of the measured bases, by which the mean base which has been used in the triangulation has been obtained; there is therefore a difference of + or −0·2 ft., or 2½ inches between the measured and computed length of these bases from the mean base.

The Hounslow Heath base was measured with Ramsden's 100 ft. steel chains, and only differs 0·173 ft., or about 2 inches, from its computed length from the mean base.

The Belhelvie base in Aberdeenshire, also measured with the steel chains, differs only 0·24 ft., or less than 3 inches, from the computed length.

The difference between the measured and computed length of the Misterton Carr base, near Doncaster, also measured with the steel chains, is only 0·157 ft., or less than 2 inches; and it will be observed that the difference between the computed and measured lengths of these three bases (measured with chains) is not greater than the difference between the measured and computed length of the Lough Foyle and Salisbury Plain bases (measured with the compensation bars), from which it may be inferred, that bases measured with steel chains are deserving of the greatest confidence; and when the great simplicity, portability, and cheapness of the chains are compared with the complex, heavy and expensive apparatus of the compensation bars, it may be anticipated that they will be more generally employed than they have been of late years, especially in the colonies, and in countries where the transport of heavy articles is effected with difficulty.

The length of the base on Rhuddlan Marsh in North Wales, which was measured with steel chains, differs 1·596 ft. from the computed length; but from the circumstance that the extremities of the base are very badly situated with reference to the surrounding Trigonometrical stations, the angles being very acute and not well observed, little confidence has been placed in the result of the comparison of its computed and measured length.

One of the first practical results arising from the completion of the triangulation is, that it is now possible to engrave the latitude and longitude on the marginal lines of the old sheets of the one-inch Map of England, and this is now being done.

The following account of the Trigonometrical operations and calculations has been drawn up by Captain Alexander R. Clarke, R.E.; this account may be considered an abridgement of that more detailed account which is now in the press, and will be shortly published.

It will be seen that the equatorial diameter of the earth, as derived from the Ordnance Survey, is 7926·610 miles, or about one mile

greater than it is given by the Astronomer Royal in his 'Figure of the Earth,' and that the ellipticity is $\frac{1}{299,33}$, or as the Astronomer Royal conjectured, something "greater than $\frac{1}{300}$," which he gives in the same paper.

The mean specific gravity of the earth, as derived from the observations at Arthur's Seat, was stated in a former paper to be 5·14; the calculations have since been revised, and it is now found to be 5·316.

The mean specific gravity of the earth, as derived from the only other observations on the attraction of mountain masses on which any reliance has been placed, viz. the Schehallien observations, is, as finally corrected by Hutton, $\frac{99}{20}$, or almost 5·0.

From the experiments with balls we have the following results:—

By Cavendish, as corrected by Baily....	5·448
By Baily..............................	5·67
By Reich..............................	5·44

From the pendulum experiments, at a great depth and on the surface, the Astronomer Royal obtained 6·566.

Two copies of the new National Standard Yard have recently been received through the Astronomer Royal, and it is obviously necessary that the geodetic measures should be given in reference to the standard; but not knowing from what scale the standard has been taken, Col. James is unable to say at present in what way the reduction is to be made; that is, whether by reference to the comparison of the old standards which have been already made, or by the mechanical process of a direct comparison of the Ordnance Standard with the new National Standard.

This introductory explanation by Col. James is followed by an account of the Trigonometrical operations and calculations; the following is a brief statement of the results:—

"1st. The four bases of verification, when their measured lengths are compared with their lengths as calculated from a mean of the Lough Foyle and Salisbury Plain bases, show the following discrepancies:—

Hounslow.	Misterton Carr.	Rhuddlan Marsh.	Belhelvie.
+0·173	−0·157	+1·596	+0·240

"2nd. The elements of the spheroid most nearly representing the surface of Great Britain are—

$$\begin{array}{lll} & \text{Feet 0.} & \text{Miles.} \\ \text{Equatorial semidiameter} & =20926249=3963{\cdot}305 & \\ \text{Polar semidiameter} & =20856337=3950{\cdot}064 & \end{array}\Bigg\}\ \text{compression} = \frac{1}{299{\cdot}33}.$$

"3rd. The elements of the spheroid most nearly representing the whole of the measured arcs considered in this paper are—

$$\begin{array}{lll} & \text{Feet 0.} & \text{Miles.} \\ \text{Equatorial semidiameter} & =20924969=3963{\cdot}064 & \\ \text{Polar semidiameter} & =20854743=3949{\cdot}760 & \end{array}\Bigg\}\ \text{compression} = \frac{1}{297{\cdot}96}.$$

"4th. The lengths of the degrees of latitude and longitude in Great Britain are as in the following table :—

Mean latitude.	From Ordnance Survey.		From the 2nd Spheroid.	
	Length in ft. of 1° of latitude.	Length in ft. of 1° of longitude.	Length in ft. of 1° of latitude.	Length in ft. of 1° of longitude.
50	364936·33	235227·42	364912·65	235215·15
51	364999·14	230312·27	364975·74	230300·33
52	365061·50	225326·39	365038·38	225314·75
53	365123·34	220271·15	365100·51	220259·79
54	365184·58	215148·11	365162·02	215137·12
55	365245·15	209958·83	365222·86	209948·14
56	365304·96	204704·93	365282·94	204694·56
57	365363·96	199387·90	365342·20	199377·84
58	365422·06	194009·37	365400·57	193999·63
59	365479·20	188571·00	365457·97	188561·57
60	365535·30	183074·50	365514·32	183065·41

The Society adjourned to Thursday, May 22nd.

May 22, 1856.

The LORD WROTTESLEY, President, in the Chair.

The following communications were read :—

I. "On the Application of Photography to the physiognomic and mental phenomena of Insanity." By HUGH W. DIAMOND, M.D. Communicated by Admiral SMYTH, For. Sec. R.S. Received April 23, 1856.

(Abstract.)

The position of the author, as Medical Superintendent of the Surrey Lunatic Asylum, has enabled him to make the peculiar application of Photography, of which he gives an account in the present communication. He points out the advantages to be derived from photographic portraits of the insane, as faithfully representing the features of the disease in its different forms, or its successive phases in the same patient, and as affording unerring records for study and comparison by the physician and psychologist. In the course of the paper frequent reference is made to the series of photographic portraits of lunatic patients with which it was accompanied.

II. "On the Problem of Three Bodies." By the Rev. J. CHALLIS, M.A., F.R.S., F.R.A.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge. Received May 15, 1856.

(Abstract.)

The object of the author is to give an approximate solution of the Problem of Three Bodies, equally applicable to the motion of the moon and to that of a planet, in which the forms of the developments of the radius-vector, longitude, and latitude in terms of the

time, are directly determined by the analysis. The solution to the first power of the disturbing force is effected by means of the following three equations, in which the letters have the significations usually given to them in the planetary theory:

$$\frac{dr^2}{dt^2}+\frac{h^2}{r^2}-\frac{2\mu}{r}+\mathrm{C}=2\int\left\{\frac{d^2\theta}{dt^2}\left(\int\frac{d\mathrm{R}}{d\theta}dt\right)-\frac{d\mathrm{R}}{dr}\frac{dr}{dt}\right\}dt$$

$$\frac{d\theta}{dt}=\frac{h}{r^2}-\frac{1}{r^2}\int\frac{d\mathrm{R}}{d\theta}dt$$

$$\frac{d^2z}{dt^2}+\frac{\mu z}{r^3}+\frac{d\mathrm{R}}{dz}=0.$$

After substituting in the right-hand side of the first equation, the values of r and θ given by a first approximation in which the disturbing force is neglected, that side becomes a known function of t. The equation can then be integrated approximately so as to give the development of r in terms of t to the first power of the disturbing force, and to any power of the eccentricity it may be thought proper to retain. By substituting in that term of the second equation which does not contain the disturbing force the value of r thus obtained, the integration of the equation gives the development of θ in terms of t, and lastly by substitution in the third equation z is similarly developed. The author has shown the practicability of this method by obtaining values of r and θ to terms of the order of the eccentricity multiplied by the disturbing force. The development of the latitude, and a more particular application of the method to the motion of the moon, are reserved for future consideration. The particular advantages of this mode of solution are, that being free from all assumption as to the forms of the developments, it gives those which are alone appropriate to the problem, and it evolves both the periodic and the secular inequalities by the same process. Terms containing *ent* as a factor, which are met with in other solutions of the same problem, do not occur in this method; but there are terms containing the factor *e'nt*, which are shown to be convertible into periodic functions, and to have reference to secular variations of the eccentricity and of the motion of the apse. The paper concludes with some general remarks on the principle of this approximate solution of the problem of three bodies, and an explanation of the analytical circumstances which make it, in common with the

method of the variation of parameters, proper for determining directly the motion of the apses of an orbit.

III. "On some of the Products of the Distillation of Boghead Coal at low temperatures." By C. GREVILLE WILLIAMS, Esq., Assistant to Dr. ANDERSON, Professor of Chemistry in the University of Glasgow. Communicated by Dr. SHARPEY, Sec. R.S. Received May 14, 1856.

In presenting a brief preliminary notice of an investigation of the substances obtained by distilling boghead coal at low temperatures, I may observe that I was induced to undertake it from remarking the low density of the naphtha produced in the process; it being only ·750 at 60° F., although its boiling-point, previous to the rectifications, was as high as 290° F.

After fifteen complete fractionations of the portion distilling below 310° F., boiling-points were obtained as low as 170°, and it was found that the fluid could be separated, by careful treatment with fuming nitric, or a mixture of nitric and sulphuric acids, into two bodies, one forming a nitro-compound, the other being unacted on. The latter was washed several times with a strong alkaline solution, and, after being digested for a few days with sticks of potash to remove adherent moisture, rectified over sodium. In this manner I obtained a colourless and very mobile fluid with a pleasant odour, distantly resembling that of hawthorn blossoms. Its density at 60° was ·725.

I selected the fraction boiling in the fifteenth rectification at 240° F. to make a preliminary experiment upon, and, after purification in the manner described, it gave in three perfectly concordant analyses, exactly the per-centage of carbon and hydrogen required for butyl (valyl of Kolbe), the radical of the butylic alcohol. Two determinations of the vapour density, taken respectively at 80° and 107° above its boiling-point, gave numbers closely coinciding with theory.

When it is considered that 68° or more of difference of boiling-point only cause a variation of 0·3 in the per-centage of carbon and hydrogen of bodies of this class, it becomes evident that if I had

taken the fraction boiling at 223° (Wurtz) or 226° (Kolbe), it would have yielded the same results. This point is now under examination.

The formula

$$C^{16} H^{18} = 4 \text{ vols.},$$

corresponds not only to butyl, but also to the hydruret of caprylyl, and, of course, both these bodies have the same vapour density; but several circumstances lead me to believe the hydrocarbon I have obtained to be the radical of the butylic alcohol. The density of the fluid, and the temperature at which it distils, are also rather in favour of this view. It will be seen that 247° F. should be the boiling-point of butyl if Frankland's determination of that of amyl be correct, and Kopp's law hold with these bodies.

A careful study of the papers already published on the radicals of this series, shows that more than one anomaly appears to exist in their physical properties, the gradations usually observed in homologous groups not being so distinctly marked as with most others, and this fact somewhat impedes their identification. The large quantity of substance which becomes at our disposal from the source mentioned, will, by facilitating the study, throw light on these points.

I believe I shall be able to isolate at least four of the radicals, viz. propyl, butyl, amyl, and caprotyl, from the coal distillate. The per-centage composition varying so little with the different homologues, I rely chiefly on vapour density and products of decomposition as the means of proving their presence.

The hydrocarbons accompanying the radicals are also quite distinct from the benzole series, as shown by the low density of the nitro-compound. The latter is extremely difficult of reduction by sulphide of ammonium or protacetate of iron, but it furnishes a volatile oily alkaloid by distillation with an alcoholic solution of potash.

The tedious purifications and the numerous operations required before the substances can be obtained in a state of sufficient purity for analysis from the coal distillate, will probably cause a considerable period to elapse before a detailed account of all the bodies can be published.

IV. "On Peristaltic Induction of Electric Currents." By Professor WILLIAM THOMSON, F.R.S. Received May 10, 1856.

Recent observations on the propagation of electricity through wires in subaqueous and subterranean telegraphic cables have brought to light phenomena of induced electric currents, which, while they are essentially different from the phenomena of what has hitherto been called electro-dynamic induction, are exactly such as might have been anticipated from the well-established theory of electrical equilibrium, had experiment afforded the data of relation between electrostatical and electro-dynamic units wanted for determining what dimensions of wire would be required to render these phenomena sensible to ordinary observation. They present a very perfect analogy with the mutual influences of a number of elastic tubes bound together laterally throughout their lengths, and surrounded and filled with a liquid which is forced through one or more of them, while the others are left with their ends open (*uninsulated*), or stopped (*insulated*), or subjected to any other particular conditions. The hydrostatic pressure applied to force the liquid through any of the tubes will cause them to swell and to press against the others, which will thus, by peristaltic action, compel the liquid contained in them to move, in different parts of them, in one direction or the other. A long solid cylinder of an incompressible elastic solid*, bored out symmetrically in four, six, or more circular passages parallel to its length, will correspond to an ordinary telegraph cable containing the same number of copper wires separated from one another only by gutta-percha: and the hydraulic motion will follow rigorously the same laws as the electrical conduction, and will be expressed by identical language in mathematics, provided the lateral dimensions of the bores are so small in comparison with their lengths, or the viscosity of the liquid so great, that the motions are not sensibly affected by *inertia*, and are consequently dependent altogether on hydrostatic pressure and fluid friction. The electrical induction now alluded to depends on the electrostatic forces determined by Coulomb; but it would be in

* Such as india-rubber very approximately is in reality.

one respect a real, and in all respects an apparent, contradiction of terms, to speak of electrostatic induction of electric currents, and I therefore venture to introduce the term *peristaltic* to characterize that kind of induction by which currents are excited in elongated conductors through the variation of electrostatic potential in the surrounding matter. On the other hand, as any inductive excitation of electric motion might be called electro-dynamic induction, it will be convenient to distinguish the kind of electro-dynamic induction first discovered by Faraday, by a distinctive name; and as the term *electro-magnetic*, which has been so applied, appears correctly characteristic, I shall call *electro-magnetic induction* that kind of action by which electric currents are excited, or inequalities of electric potential sustained, in a conductor of electricity, by variations of magnetic or electro-magnetic potential, or by absolute or relative motion of the conductor itself across lines of magnetic or electro-magnetic force.

The most general problem of peristaltic induction is to determine the motion of electricity in any number of long conducting wires, insulated from one another within an uninsulated tube of conducting material, when subjected each to any prescribed electrical action at its extremities; without supposing any other condition regarding the sections and relative dispositions of the conductors than—(1), that their lateral dimensions and mutual distances are so small in proportion to their lengths, that the effects of peristaltic induction are paramount over those of electro-magnetic induction; and (2), that the section of the entire system of conductors, if not uniform in all parts, varies so gradually as to be sensibly uniform through every part of the length not a very large multiple of the largest lateral dimension. In the present communication I shall only give the general equations of motion by which the physical conditions to be satisfied are expressed for every case; and I shall confine the investigation of solutions to certain cases of uniform and symmetrical arrangement, such as are commonly used in the submarine telegraph cable.

At any time t, let q_1, q_2, q_3, &c. be the quantities of electricity with which the different wires are charged, per unit of length of each, at a distance x from one extremity, O, of the conducting system; and let v_1, v_2, v_3, &c. be the electrostatical potentials in the same parts of those conductors. Let $\varpi_1^{(1)}$, $\varpi_1^{(2)}$, $\varpi_1^{(3)}$, &c., $\varpi_2^{(1)}$, $\varpi_2^{(2)}$, $\varpi_2^{(3)}$, &c., $\varpi_3^{(1)}$, $\varpi_3^{(2)}$, $\varpi_3^{(3)}$, &c. be coefficients, such that the electro-

statical potentials (v_1, v_2, &c.), due to stated charges (q_1, q_2, &c.) of the different wires, are expressed by the equations

$$\left.\begin{array}{l} v_1 = \varpi_1^{(1)} q_1 + \varpi_1^{(2)} q_2 + \varpi_1^{(3)} q_3 + \text{\&c.} \\ v_2 = \varpi_2^{(1)} q_1 + \varpi_2^{(2)} q_2 + \varpi_2^{(3)} q_3 + \text{\&c.} \\ v_3 = \varpi_3^{(1)} q_1 + \varpi_3^{(2)} q_2 + \varpi_3^{(3)} q_3 + \text{\&c.} \\ \quad \text{\&c.} \qquad\qquad \text{\&c.} \end{array}\right\} \quad . \quad . \quad . \quad (1).$$

If the sections of all the conductors are circular, these coefficients ($\varpi_1^{(1)}$, $\varpi_1^{(2)}$, &c.) may be easily determined numerically to any required degree of accuracy, in each particular case, by the *method of electro-statical images*. The electromotive force per unit of length at the position x will be, in the different wires,

$$\frac{dv_1}{dx}, \quad \frac{dv_2}{dx}, \quad \frac{dv_3}{dx},$$

respectively, and therefore if γ_1, γ_2, γ_3, &c. denote the strength of current at the same position, and k_1, k_2, k_3, &c. the resistances to conduction per unit of length in the different wires respectively, we have by the law of Ohm, applied to the action of peristaltic electro-motive force,

$$k_1\gamma_1 = -\frac{dv_1}{dx}, \quad k_2\gamma_2 = -\frac{dv_2}{dx}, \quad k_3\gamma_3 = -\frac{dv_3}{dx} \quad . \quad . \quad (2).$$

Now unless the strength of current be uniform along any one of the wires, the charge of electricity will experience accumulation or diminution in any part of it by either more or less electricity flowing in on one side than out on the other; and the mathematical expression of these circumstances is clearly

$$\frac{dq_1}{dt} = -\frac{d\gamma_1}{dx}, \quad \frac{dq_2}{dt} = -\frac{d\gamma_2}{dx}, \quad \frac{dq_3}{dt} = -\frac{d\gamma_3}{dx} \quad . \quad . \quad . \quad (3).$$

Using in these equations the values of γ_1, γ_2, γ_3, &c. given by (2), and then substituting for v_1, v_2, v_3, &c. their expressions (1), we obtain

$$\left.\begin{array}{l} \dfrac{dq_1}{dt} = \dfrac{d}{dx}\left\{\dfrac{1}{k_1}\cdot\dfrac{d(\varpi_1^{(1)}q_1)}{dx} + \dfrac{1}{k_2}\dfrac{d(\varpi_1^{(2)}q_2)}{dx} + \dfrac{1}{k_3}\dfrac{d(\varpi_1^{(3)}q_3)}{dx} + \text{\&c.}\right\} \\ \dfrac{dq_2}{dt} = \dfrac{d}{dx}\left\{\dfrac{1}{k_1}\cdot\dfrac{d(\varpi_2^{(1)}q_1)}{dx} + \dfrac{1}{k_2}\cdot\dfrac{d(\varpi_2^{(2)}q_2)}{dx} + \dfrac{1}{k_3}\dfrac{d(\varpi_2^{(3)}q_3)}{dx} + \text{\&c.}\right\} \\ \dfrac{dq_3}{dt} = \dfrac{d}{dx}\left\{\dfrac{1}{k_1}\cdot\dfrac{d(\varpi_3^{(1)}q_1)}{dx} + \dfrac{1}{k_2}\cdot\dfrac{d(\varpi_3^{(2)}q_2)}{dx} + \dfrac{1}{k_3}\dfrac{d(\varpi_3^{(3)}q_3)}{dx} + \text{\&c.}\right\} \\ . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \end{array}\right\} (4),$$

which are the general equations of motion required.

It is to be observed that k_1, k_2, &c., $\varpi_1^{(1)}$, $\varpi_1^{(2)}$, $\varpi_2^{(1)}$, &c. will be functions of x if the section of the conducting system is heterogeneous in different positions along it; but in all cases in which each conductor is uniform, and uniformly situated with reference to the others along the whole length, these coefficients will be constant, and the equations become reduced to

$$\left.\begin{aligned}\frac{dq_1}{dt}&=\frac{\varpi_1^{(1)}}{k_1}\frac{d^2q_1}{dx^2}+\frac{\varpi_1^{(2)}}{k_2}\frac{d^2q_2}{dx^2}+\frac{\varpi_1^{(3)}}{k_3}\frac{d^2q_3}{dx^2}+\text{\&c.}\\ \frac{dq_2}{dt}&=\frac{\varpi_2^{(1)}}{k_1}\frac{d^2q_1}{dx^2}+\frac{\varpi_2^{(2)}}{k_2}\frac{d^2q_2}{dx^2}+\frac{\varpi_2^{(3)}}{k_3}\frac{d^2q_3}{dx^2}+\text{\&c.}\\ \frac{dq_3}{dt}&=\frac{\varpi_3^{(1)}}{k_1}\frac{d^2q_1}{dx^2}+\frac{\varpi_3^{(2)}}{k_2}\frac{d^2q_2}{dx^2}+\frac{\varpi_3^{(3)}}{k_3}\frac{d^2q_3}{dx^2}+\text{\&c.}\\ &\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\end{aligned}\right\}\quad . \quad . \quad (5).$$

The most obvious general method of treatment for integrating these equations, is to find elementary solutions by assuming

$$q_1=A_1u,\quad q_2=A_2u,\quad q_3=A_3u, \ldots\ldots q_i=A_iu, \quad . \quad . \quad (6),$$

where u satisfies the equation

$$\frac{du}{dt}=\varkappa\frac{d^2u}{dx^2}\quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7).$$

This will reduce the differential equations (5) to a set of linear equations among the coefficients A_1, A_2, ... A_i, giving by elimination an algebraic equation of the ith degree having i real roots, to determine $\varkappa$. The particular form of elementary solution of the equation (7) to be used may be chosen from among those given by Fourier, according to convenience, for satisfying the terminal conditions for the different wires.

In thinking on some applications of the preceding theory, I have been led to consider the following general question regarding the mutual influence of electrified conductors:—If, of a system of detached insulated conductors, one only be electrified with a given absolute charge of electricity, *will the potential excited in any one of the others be equal to that which the communication of an equal absolute charge to this other would excite in the first?* I now find that a general theorem communicated by myself to the Cambridge Mathematical Journal, and published in the Numbers for November 1842 and February 1843, but, as I afterwards (Jan. 1845) learned, first given by Green in his Essay on the Mathematical Theory of

Electricity and Magnetism (Nottingham, 1828), leads to an affirmative answer to this question.

The general theorem to which I refer is, that if, considering the forces due respectively to two different distributions of matter (whether real, or such as is imagined in theories of electricity and magnetism), we denote by N_1, N_2 their normal components at any point of a closed surface, or group of closed surfaces, S, containing all parts of each distribution of matter, and by V_1, V_2 the potentials at the same point due respectively to the two distributions, and if ds be an element of the surface S, the value of $\iint N_1 V_2 ds$ is the same as that of $\iint N_2 V_1 ds$ (each being equal to the integral $\iiint R_1 R_2 \sin\theta \, dx \, dy \, dz$ extended over the whole of space external to the surface S, at any point (x, y, z) of which external space the two resultants are denoted by R_1, R_2 respectively, and the angle between their directions by θ). To apply this with reference to the proposed question, let the first distribution of matter consist of a certain charge, q, communicated to one of a group of insulated conductors, and the inductive electrifications of the others, not one of which has any absolute charge; let the second distribution of matter consist of the electrifications of the same group of conductors when an equal quantity q is given to a second of them, and all the others are destitute of absolute charges; and let surface S be the group of the surfaces of the different conductors. Since the potential is constant through each separate conductor, the integral $\iint N_1 V_2 \, ds$ will be equal to the sum of a set of terms of the form $[V_2][\iint N_1 ds]$, where $[V_2]$ denotes the value in any of these conductors of the potential of the second distribution, and $[\iint N_1 ds]$ an integral including the whole surface of the same conductor, but no part of that of any of the others. Now by a well-known theorem, first given by Green, $[\iint N_1 ds]$ is equal to $4\pi q$ if q denote the absolute quantity of matter within the surface of the integral (as is the case for the first group of conductors), and vanishes if there be no distribution of matter, or (as is the case with each of the other conductors) if there be equal quantities of positive and negative matter within the surface over which the integral is extended. Hence if $[V_2]_1$ denote the potential in the first conductor due to the second distribution of matter, we have

$$\iint N_1 V_2 ds = 4\pi [V_2]_1 q.$$

Similarly, we have

$$\iint N_2 V_1 ds = 4\pi [V_1]_2 q.$$

Hence, by the general theorem, we conclude $[V_2]_1 = [V_1]_2$, and so demonstrate the affirmative answer to the question stated above.

I think it unnecessary to enter on details suited to the particular case of lateral electrostatic influence between neighbouring parts of a number of wires insulated from one another under a common conducting sheath, when uniform or varying electric currents are sent through by them; for which a particular demonstration in geometry of two dimensions, analogous to the demonstration of Green's theorem to which I have referred as involving the consideration of a triple integral for space of three dimensions, may be readily given; but, as a particular case of the general theorem I have now demonstrated, it is obviously true that the potential in one wire due to a certain quantity of electricity per unit of length in the neighbouring parts of another under the same sheath, is equal to the potential in this other, due to an equal electrification of the first.

Hence the following relations must necessarily subsist among the coefficients of mutual peristaltic induction in the general equations given above,

$$\varpi_1^{(2)} = \varpi_2^{(1)}; \quad \varpi_1^{(3)} = \varpi_3^{(1)}; \quad \varpi_2^{(3)} = \varpi_3^{(2)}; \text{ \&c.}$$

On the Solution of the Equations of Peristaltic Induction in symmetrical systems of Submarine Telegraph Wires.

The general method which has just been indicated for resolving the equations of electrical motion in any number of linear conductors subject to mutual peristaltic influence, fails when these conductors are symmetrically arranged within a symmetrical conducting sheath (and therefore actually in the case of any ordinary multiple wire telegraph cable), from the determinantal equation having sets of equal roots. Regular analytical methods are well known by which the solutions for such particular cases may be derived from the failing general solutions; but it is nevertheless interesting to investigate each particular case specially, so as to obtain its proper solution by a synthetical process, the simplest possible for the one case considered alone. In the present communication, the problem of peristaltic induction is thus treated for some of the most common cases of actual submarine telegraph cables, in which two or more wires of equal dimen-

sions are insulated in symmetrical positions within a cylindrical conducting sheath of circular section.

CASE I.—*Two-wire Cable.*

In the general equations (according to the notation of the first part of this communication) we have $k_1=k_2$; $\varpi_1^{(1)}=\varpi_2^{(2)}$; and $\varpi_2^{(1)}=\varpi_1^{(2)}$: and it will be convenient now to denote the values of the members of these three equations by k, $\frac{1}{c}$, and $\frac{f}{c}$ respectively; that is, to express by k the galvanic resistance in each wire per unit of length, by c the electrostatical capacity of each per unit of length when the other is prevented from acquiring an absolute charge, and by f the proportion in which this exceeds the electrostatical capacity of each when the other has a charge equal to its own; or in other words, to assume c and f so that

$$\left.\begin{aligned} v_1&=\frac{1}{c}q_1+\frac{f}{c}q_2 \\ v_2&=\frac{f}{c}q_1+\frac{1}{c}q_2 \end{aligned}\right\} \quad \dots\dots\dots \quad (1),$$

if v_1 and v_2 be the potentials in the two wires in any part of the cable where they are charged with quantities of electricity respectively q_1 and q_2 per unit of length. The equations of electrical conduction along the two wires then become

$$\left.\begin{aligned} \frac{dv_1}{dt}&=\frac{1}{kc}\left(\frac{d^2v_1}{dx^2}+f\frac{d^2v_2}{dx^2}\right) \\ \frac{dv_2}{dt}&=\frac{1}{kc}\left(f\frac{d^2v_1}{dx^2}+\frac{d^2v_2}{dx^2}\right) \end{aligned}\right\} \quad \dots\dots \quad (2).$$

From these we have, by addition and subtraction,

$$\frac{d\vartheta}{dt}=\frac{1+f}{kc}\frac{d^2\vartheta}{dx^2}, \text{ and } \frac{d\omega}{dt}=\frac{1-f}{kc}\frac{d^2\omega}{dx^2} \quad \dots \quad (3),$$

where ϑ and ω are such that

$$v_1=\vartheta+\omega, \quad v_2=\vartheta-\omega \quad \dots\dots\dots \quad (4).$$

If both wires reached to an infinite distance in each direction, the conditions to be satisfied in integrating the equations of motion would be simply that the initial distribution of electricity along each must be whatever is prescribed; that is, that

$$\left.\begin{aligned} &v_1=\varphi_1(x), \text{ and } v_2=\varphi_2(x) \\ \text{when} \quad &t=0 \end{aligned}\right\} \quad \dots\dots \quad (5),$$

φ_1 and φ_2 denoting two arbitrary functions. Hence, according to Fourier, we have, for the integrals of the equations (3),

$$\left.\begin{aligned}\vartheta&=\sqrt{\frac{kc}{4(1+f)\pi}}\,.\,t^{-\frac{1}{2}}\int_{-\infty}^{\infty}\frac{1}{2}\{\varphi_1(\xi)+\varphi_2(\xi)\}\varepsilon^{-\frac{kc(\xi-x)^2}{4(1+f)t}}\,d\xi\\ \omega&=\sqrt{\frac{kc}{4(1-f)\pi}}\,t^{-\frac{1}{2}}\int_{-\infty}^{\infty}\frac{1}{2}\{\varphi_1(\xi)-\varphi_2(\xi)\}\varepsilon^{-\frac{kc(\xi-x)^2}{4(1-f)t}}\,d\xi\end{aligned}\right\}\quad(6),$$

and the solution of the problem is expressed in terms of these integrals by (4).

If now we suppose the cable to have one end at a finite distance from the part considered, for instance at the point O from which x is reckoned, and if at this end each wire is subjected to electric action so as to make its potential vary arbitrarily with the time, there will be the additional condition

$$\left.\begin{aligned}&v_1=\psi_1(t),\text{ and }v_2=\psi_2(t),\\ \text{when}\quad &x=0\end{aligned}\right\}\quad.\quad.\quad.\quad.\quad.\quad.\quad(7),$$

to be fulfilled. In the other conditions, (5), only positive values of x have now to be considered, but they must be fulfilled in such a way as not to interfere with the prescribed values of the potentials at the ends of the wires; which may be done according to the principle of images, by still supposing the wires to extend indefinitely in both directions, and in the beginning to be symmetrically electrified with contrary electricities on the two sides of O. To express the new condition (7), a form of integral, investigated in a communication to the Royal Society ('Proceedings,' May 10, 1855, p. 385), may be used; and we thus have for the integrals of equations (3),

$$\left.\begin{aligned}\vartheta=\sqrt{\frac{kc}{4(1+f)\pi}}\Bigg[&t^{-\frac{1}{2}}\int_0^{\infty}\frac{1}{2}\{\varphi_1(\xi)+\varphi(\xi)\}\{\varepsilon^{-\frac{kc(\xi-x)^2}{4(1+f)t}}-\varepsilon^{-\frac{kc(\xi+x)^2}{4(1+f)t}}\}d\xi\\ &+x\int_0^t\frac{1}{2}\{\psi_1(\theta)+\psi_2(\theta)\}\varepsilon^{-\frac{kcx^2}{4(1+f)(t-\theta)}}\frac{d\theta}{(t-\theta)^{\frac{3}{2}}}\Bigg]\\ \omega=\sqrt{\frac{kc}{4(1-f)\pi}}\Bigg[&t^{-\frac{1}{2}}\int_0^{\infty}\frac{1}{2}\{\varphi_1(\xi)\varphi_2-(\xi)\}\{\varepsilon^{-\frac{kc(\xi-x)^2}{4(1-f)t}}-\varepsilon^{-\frac{kc(\xi+x)^2}{4(1-f)t}}\}d\xi\\ &+x\int_0^t\frac{1}{2}\{\psi_1(\theta)+\psi_2(\theta)\}\varepsilon^{-\frac{kcx^2}{4(1-f)(t-\theta)}}\frac{d\theta}{(t-\theta)^{\frac{3}{2}}}\Bigg]\end{aligned}\right\}\quad(8).$$

Lastly, instead of the cable extending indefinitely on one side of

the end O, let it be actually limited at a point E. If the ends of the two wires at E be subjected to electric action, so as to make each vary arbitrarily with the time, the new conditions to be satisfied, in addition to the others, (5) and (7), will be

$$\left.\begin{array}{ll} & v_1=\chi_1(t) \text{ and } v_2=\chi_2(t) \\ \text{when} & x=a \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9),$$

if χ_1 and χ_2 denote two arbitrary functions, and a the length OE. Or, on the other hand, if they be connected together, so that a current may go from O to E along one and return along the other, the new conditions will be

$$\left.\begin{array}{ll} & v_1-v_2=0,\ \dfrac{d(v_1+v_2)}{dx}=0, \\ \text{when} & x=a \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)'.$$

Either of these requirements may be fulfilled in an obvious way by the *method of successive images*, and we so obtain the following respective solutions :—

$$\left.\begin{aligned} \vartheta=&\sqrt{\frac{kc}{4(1+f)\pi}}\Big[t^{-\frac{1}{2}}\int_0^a\frac{1}{2}\{\phi_1(\xi)+\phi_2(\xi)\}\mathrm{F}_{(f)}(\xi,t)d\xi \\ &+\int_0^t\frac{d\theta}{(t-\theta)^{\frac{3}{2}}}\Big[\frac{1}{2}\{\psi_1(\theta)+\psi_2(\theta)\}\mathfrak{F}_{(f)}(x,t-\theta)+\frac{1}{2}\{\chi_1(\theta)+\chi_2(\theta)\}\mathfrak{F}(a-x,t-\theta)\Big]\Big] \\ \omega=&\sqrt{\frac{kc}{4(1-f)\pi}}\Big[t^{-\frac{1}{2}}\int_0^a\frac{1}{2}\{\phi_1(\xi)-\phi_2(\xi)\}\mathrm{F}_{(-f)}(\xi,t)d\xi \\ &+\int_0^t\frac{d\theta}{(t-\theta)^{\frac{3}{2}}}\Big[\frac{1}{2}\{\psi_1(\theta)-\psi_2(\theta)\}\mathfrak{F}_{(-f)}(x,t-\theta)+\frac{1}{2}\{\chi_1(\theta)-\chi_2(\theta)\}\mathfrak{F}_{(-f)}(a-x,t-\theta)\Big]\Big] \end{aligned}\right\}(10)$$

$$\left.\begin{aligned} \vartheta=&\sqrt{\frac{kc}{4(1+f)\pi}}\Big[t^{-\frac{1}{2}}\int_0^a\frac{1}{2}\{\phi_1(\xi)+\phi_2(\xi)\}\mathrm{E}_{(f)}(\xi,t)d\xi \\ &\qquad+\int_0^t\frac{1}{2}\{\psi_1(\theta)+\psi_2(\theta)\}\mathfrak{E}_{(f)}(x,t-\theta)\frac{d\theta}{(t-\theta)^{\frac{3}{2}}}\Big] \\ \omega=&\sqrt{\frac{kc}{4(1-f)\pi}}\Big[t^{-\frac{1}{2}}\int_0^a\frac{1}{2}\{\phi_1(\xi)-\phi_2(\xi)\}\mathrm{F}_{(-f)}(\xi,t)d\xi \\ &\qquad+\int_0^t\frac{1}{2}\{\psi_1(\theta)-\psi_2(\theta)\}\mathfrak{F}_{(-f)}(x,t-\theta)\frac{d\theta}{(t-\theta)^{\frac{3}{2}}}\Big] \end{aligned}\right\}(10)',$$

where F, $\mathfrak{F}$, E, $\mathfrak{E}$ denote for brevity the following functions :—

$$\left.\begin{aligned}
F_{(f)}(\xi, t) &= \sum_{i=-\infty}^{i=\infty} \left\{ \varepsilon^{-\frac{kc(x+2ia-\xi)^2}{4(1+f)t}} - \varepsilon^{-\frac{kc(x+2ia+\xi)^2}{4(1+f)t}} \right\} \\
\mathfrak{F}_{(f)}(x, t-\theta) &= \sum_{i=-\infty}^{i=\infty} (x+2ia)\varepsilon^{-\frac{kc(x+2ia)^2}{4(1+f)(t-\theta)}} = \frac{(1+f)(t-\theta)}{kc}\{F_{(f)}(\xi, t-\theta) \div \xi\}_{\xi=0} \\
E_{(f)}(\xi, t) &= \sum_{-\infty}^{\infty} (-1)^i \left\{ \varepsilon^{-\frac{kc(x+2ia-\xi)^2}{4(1+f)t}} - \varepsilon^{-\frac{kc(x+2ia+\xi)^2}{4(1+f)t}} \right\} \\
\mathfrak{E}_{(f)}(x, t-\theta) &= \sum_{-\infty}^{\infty} (-1)^i (x+2ia)\varepsilon^{-\frac{kc(x+2ia)^2}{4(1+f)(t-\theta)}} = \frac{(1+f)(t-\theta)}{kc}\{E_{(f)}(\xi, t-\theta) \div \xi\}_{\xi=0}
\end{aligned}\right\} (11).$$

Each of the functions F and E is clearly the difference between two periodical functions of $(\xi - x)$ and $(\xi + x)$; and each of the functions $\mathfrak{F}$ and $\mathfrak{E}$ is a periodical function of x simply. The expressions for these four functions, obtained by the ordinary formulæ for the expression of periodical functions in trigonometrical series, are as follows:—

$$\left.\begin{aligned}
F_{(f)}(\xi, t) &= \frac{2}{a}\sqrt{\frac{4(1+f)\pi t}{kc}} \sum_{i=1}^{i=\infty} \varepsilon^{-\frac{i^2\pi^2(1+f)t}{a^2kc}} \sin\frac{i\pi x}{a}\sin\frac{i\pi\xi}{a} \\
\mathfrak{F}_{(f)}(x, t-\theta) &= \frac{1}{2a^2}\left[\frac{4(1+f)\pi(t-\theta)}{kc}\right]^{\frac{3}{2}} \sum_{1}^{\infty} i\varepsilon^{-\frac{i^2\pi^2(1+f)(t-\theta)}{a^2kc}} \sin\frac{i\pi x}{a} \\
E_{(f)}(\xi, t) &= \frac{2}{a}\sqrt{\frac{4(1+f)\pi t}{kc}} \sum_{1}^{\infty} \varepsilon^{-\frac{(2i-1)^2\pi^2(1+f)t}{4a^2kc}} \sin\frac{(2i-1)\pi x}{2a}\sin\frac{(2i-1)\pi\xi}{2a} \\
\mathfrak{E}_{(f)}(x, t-\theta) &= \frac{1}{4a^2}\left[\frac{4(1+f)\pi(t-\theta)}{kc}\right]^{\frac{3}{2}} \sum_{1}^{\infty} (2i-1)\varepsilon^{-\frac{(2i-1)^2\pi^2(1+f)(t-\theta)}{4a^2kc}} \sin\frac{(2i-1)\pi x}{2a}
\end{aligned}\right\} (12).$$

Either (11) or (12) may be used to obtain explicit expressions for the solutions (10) and (10)′, in convergent series; but of the series so obtained, (11) converge very rapidly and (12) very slowly when t is small; and, on the contrary, (11) very slowly and (12) very rapidly when t is large. It is satisfactory, that, as t increases, the first set of series (11) do not cease to be, before the second set (12) become, convergent enough to be extremely convenient for practical computation.

The solutions obtained by using (12), in (10) and (10)′, are the same as would have been found by applying Fourier's ordinary process to derive from the elementary integral $\varepsilon^{-mt}\sin nx$ the effects of the initial arbitrary electrification of the wires, and employing a

method given by Professor Stokes* to express the effects of the variations arbitrarily applied at the free ends of the wires.

CASE II.—*Three-wire Cable.*

The equations of mutual influence between the wires may be clearly put under the forms

$$cv_1=q_1+f(q_2+q_3),\quad cv_2=q_2+f(q_3+q_1),\quad cv_3=q_3+f(q_1+q_2)\,;$$

and the equations of electrical motion along them are then as follows:—

$$kc\frac{dq_1}{dt}=\frac{d^2q_1}{dx^2}+f\left(\frac{d^2q_2}{dx^2}+\frac{d^2q_3}{dx^2}\right),\quad kc\frac{dq_2}{dt}=\frac{d^2q_2}{dx^2}+f\left(\frac{d^2q_3}{dx^2}+\frac{d^2q_1}{dx^2}\right),$$

$$kc\frac{dq_3}{dt}=\frac{d^2q_3}{dx^2}+f\left(\frac{d^2q_1}{dx^2}+\frac{d^2q_2}{dx^2}\right).$$

If we assume

$$\sigma=q_1+q_2+q_3,\ \omega_1=2q_1-q_2-q_3,\ \omega_2=2q_2-q_3-q_1,\ \omega_3=2q_3-q_1-q_2,$$

which give

$$q_1=\frac{1}{3}\sigma+\omega_1,\quad q_2=\frac{1}{3}\sigma+\omega_2,\quad q_3=\frac{1}{3}\sigma+\omega_3,$$

and require that $\omega_1+\omega_2+\omega_3=0$, we find by addition and subtraction, among the equations of conduction,

$$kc\frac{d\sigma}{dt}=(1+2f)\frac{d^2\sigma}{dx^2}$$

and

$$kc\frac{d\omega}{dt}=(1-f)\frac{d^2\omega}{dx^2},$$

where for ω may be substituted either ω_1, ω_2, or ω_3.

CASE III.—*Four-wire Cable.*

The equations of mutual influence being

$$cv_1=q_1+f(q_2+q_4)+gq_3,$$

and other four symmetrical with this; and the equations of motion,

$$kc\frac{dq_1}{dt}=\frac{d^2q_1}{dx^2}+f\left(\frac{d^2q_2}{dx^2}+\frac{d^2q_4}{dx^2}\right)+g\,\frac{d^2q_3}{dx^2},$$

&c. &c. &c.,

* See Cambridge Phil. Trans. vol. viii. p. 533, "On the Critical Values of the sums of Periodic Series."

we may assume

$$q_1+q_2+q_3+q_4=\sigma, \quad q_1-q_3=\omega_1,$$
$$q_1-q_2+q_3-q_4=\vartheta, \quad q_2-q_4=\omega_2;$$

which give

$$q_1=\frac{1}{4}(\sigma+\vartheta+2\omega_1); \quad q_2=\frac{1}{4}(\sigma-\vartheta+2\omega_2);$$

$$q_3=\frac{1}{4}(\sigma+\vartheta-2\omega_1); \quad q_4=\frac{1}{4}(\sigma-\vartheta-2\omega_2);$$

and we find from the equations of conduction,

$$kc\frac{d\sigma}{dt}=(1+2f+g)\frac{d^2\sigma}{dx^2}; \; kc\frac{d\vartheta}{dt}=(1-2f+g)\frac{d^2\vartheta}{dx^2}; \; kc\frac{d\omega}{dt}=(1-g)\frac{d^2\omega}{dx^2}.$$

Case IV.—*Cable of six wires symmetrically arranged.*

Equations of mutual influence,

$$cv_1=q_1+f(q_2+q_6)+g(q_3+q_5)+hq_4$$
$$\&c. \qquad \&c. \qquad \&c.$$

Equations of conduction,

$$kc\frac{dq_1}{dt}=\frac{d^2q_1}{dx^2}+f\left(\frac{d^2q_2}{dx^2}+\frac{d^2q_6}{dx^2}\right)+g\left(\frac{d^2q_3}{dx^2}+\frac{d^2q_5}{dx^2}\right)+h\frac{d^2q_4}{dx^2}.$$

Then assuming

$$q_1+q_2+q_3+q_4+q_5+q_6=\sigma$$
$$q_1-q_4+q_3-q_6+q_5-q_2=\vartheta$$
$$3(q_1+q_4)-\sigma=\omega_1; \quad 3(q_3+q_6)-\sigma=\omega_2; \quad 3(q_5+q_2)-\sigma=\omega_3;$$
$$3(q_1-q_4)-\vartheta=\rho_1; \quad 3(q_3-q_6)-\vartheta=\rho_2; \quad 3(q_5-q_2)-\vartheta=\rho_3;$$

which require that

$$\omega_1+\omega_2+\omega_3=0, \text{ and } \rho_1+\rho_2+\rho_3=0;$$

we have

$$kc\frac{d\sigma}{dt}=[1+2(f+g)+h]\frac{d^2\sigma}{dx^2}; \quad kc\frac{d\vartheta}{dt}=[1-2(f-g)-h]\frac{d^2\vartheta}{dx^2};$$

$$kc\frac{d\omega}{dt}=[1-(f+g)+h]\frac{d^2\omega}{dx^2}; \quad kc\frac{d\rho}{dt}=[1+(f-g)-h]\frac{d^2\rho}{dx^2}.$$

These equations, integrated by the usual process to fulfil the prescribed conditions, determine σ, ϑ, ω_1, ω_2, ω_3, ρ_1, ρ_2, ρ_3; and we then have, for the solution of the problem,

$$q_1=\frac{1}{6}(\sigma+\vartheta+\omega_1+\rho_1); \; q_3=\frac{1}{6}(\sigma+\vartheta+\omega_2+\rho_2); \; q_5=\frac{1}{6}(\sigma+\vartheta+\omega_3+\rho_3);$$

$$q_4=\frac{1}{6}(\sigma-\vartheta+\omega_1-\rho_1); \; q_6=\frac{1}{6}(\sigma-\vartheta+\omega_2-\rho_2); \; q_2=\frac{1}{6}(\sigma-\vartheta+\omega_3-\rho_3).$$

V. "Experimental Researches on the Functions of the Mucous Membrane of the Gall-bladder, principally with reference to the Conversion of *Hepatic* into *Cystic* Bile." By GEORGE KEMP, M.D. Cantab. Communicated by the Rev. W. CLARK, M.D., F.R.S., Professor of Anatomy in the University of Cambridge. Received May 1, 1856.

(Abstract.)

Referring to the well-known difference in taste and other physical properties between the bile as it immediately proceeds from the liver and the same fluid after it has been retained for a time in the gall-bladder, the author observes, that the nature of this difference and the agency by which it is effected, are questions which have not yet met with the attention they deserve, and that he had accordingly been led to make them the subject of experimental inquiry. As, however, it is only on rare occasions that the *hepatic* bile can be procured in quantity sufficient for chemical experiment, and then only at the risk of its being altered by pathological conditions of the secreting organ, the author considers that, however clearly individual facts on the subject may be demonstrated, any deductions made therefrom must be referred to the lower department of probable evidence; and it is with this reservation that he lays his conclusions before the Royal Society, whilst, at the same time, he believes that, so far as the nature of the case admits, he has been able to elicit a new fact respecting the mucous membrane of the gall-bladder, which may lead to the better comprehension of the functions of mucous membranes generally.

Assuming, in the first place, that the change in properties which the bile undergoes in the gall-bladder is brought about either by the mucous secretion of that reservoir, or by some operation exerted by its internal membrane, it is observed, with respect to the action of the mucus, 1st, that when left in the gall-bladder in contact with the cystic bile, it is capable of subverting the composition of that fluid. 2nd, That this change is much accelerated by even a moderately elevated temperature. 3rd, That when the contents of the gall-bladder are evaporated to a syrupy consistence, the bile, at first

neutral, becomes alkaline and broken up into several organic groups. 4th, That if the mucus of the gall-bladder be carefully removed by alcohol or acetic acid, and the perfectly fresh bile be then evaporated, these changes do not take place.

From these facts it follows that the mucus of the gall-bladder is a highly catalytic body, and that the analysis of bile which has been left in contact with it, under the conditions above stated, must lead to varying and unsatisfactory results.

From these considerations it naturally occurred to try the effect of placing the mucus of the gall-bladder in contact with hepatic bile; but the experiment was not performed, as it was found impracticable to obtain the mucus of the gall-bladder free from cystic bile without precipitation by reagents. Desiring, however, to ascertain whether the mucus is principally retained in contact with the inner surface of the gall-bladder or diffused through its contents, the author subjected the gall-bladder and its contained fluid, taken from an ox just slaughtered, to a freezing mixture of snow and salt, until all but the central part of the fluid was frozen, and on pouring out the latter found it to contain mucus; thus showing that this secretion is not merely confined to the inner surface of the gall-bladder for the purpose of protection and lubrication of the subjacent membrane, but is diffused throughout the bile contained in that reservoir.

Leaving now the mucous secretion, the object was first to ascertain whether the mucous membrane itself possesses the property of changing the molecular structure of the bile; then to observe whether it possesses any analogy to other mucous membranes, such as the epithelial membrane of the calf's stomach, &c., in acting upon animal fluids and solutions of bodies which readily break up into binary forms; to examine, in the next place, its action upon albumen, the white of an egg being the substance selected; and, finally, to determine its effects on the biliary secretion, as produced in the liver before its admission into the gall-bladder.

Action of the Mucous Membrane of the Gall-bladder upon Bile.

December 11, 1855.—The mucous membrane of the gall-bladder was dissected, or rather stripped from the other portion of the viscus, and washed in several waters, until the mucous secretion and bile disappeared; a small portion was now placed in an evaporating dish

and covered with fresh ox-bile, from which the mucus had been carefully removed; the bile also was tested with diluted acetic acid, giving no precipitate or appearance of turbidity. The whole was, at 1 P.M., placed in a warm situation. At 3 P.M. the solution was tested with acetic acid, when it became densely turbid. Temperature, 43°·5 C. The peculiar odour of musk was very distinct in this solution, though not perceptible in the remainder of the fresh bile. A small quantity of water was added, to replace that lost by evaporation, and the solution returned to its warm situation. 6 P.M. again tested, with similar results; the musky odour very strong. 7·30 P.M. again tested, when not only was the solution rendered turbid, but a white precipitate was thrown down; the fluid was also distinctly alkaline.

The mucous lining of the gall-bladder is therefore a catalytic body capable of producing molecular changes in the bile.

Action of the Mucous Membrane of the Gall-bladder upon certain Bodies which readily break up into Organic Groups.

Milk. December 18.—A portion of mucous membrane was scraped and washed with scrupulous care, in order that the whole of the mucus might be removed; it was then covered with fresh milk and exposed to a temperature of 32° C. In one hour the fluid was separated into serum, turbid with caseine, and an over-stratum of a creamy substance greatly resembling butter, thus exhibiting a catalytic influence very analogous to the action of rennet. The whole contents of the evaporating dish were then set aside, as having effected the object of the experiment; on the 21st, however, the author was induced to taste the fluid, and, to his astonishment, found it intensely bitter, and, what is more remarkable, on applying Pettenkofer's well-known test of sugar and sulphuric acid, with increase of temperature, the characteristic rose-tint was developed. The musky smell was not observable.

Honey. December 26.—A portion of mucous membrane, carefully washed, was covered with a solution of honey (one measure of honey to three of water). After six hours' exposure at 38° C., it was found bitter, and gave a precipitate with diluted acetic acid; this was not the case with the original solution. The fluid, being slightly acid, was carefully neutralized with carbonate of soda. The taste,

after another period of six hours, was very bitter. On the 28th the fluid had evaporated down to a thick honey consistence.

The next remark on this subject, in the author's rough notes taken at the time, is the following:—

January 27.—"The honey solution is now nearly evaporated. A mass of crystals (grape-sugar) with a small quantity of syrup, intensely bitter." The syrup could be readily poured off from the crystals. The mucous membrane was not in the slightest degree decomposed; swelled and elastic, not splitting into layers.

It is well known that, after long keeping, granules of grape-sugar are found in honey; therefore, on the 22nd of April, the honey from which the experiments were made was re-examined, and found to be nearly homogeneous and not separated into crystals and syrup; indeed the whole physical appearances are so different from the honey after being operated upon, that the author cannot doubt the influence of the membrane in effecting the changes registered.

At this stage of the inquiry an important doubt suggested itself. In the above experiments no small importance has been attached to the circumstance of bitterness becoming developed in the various solutions when kept in contact with the membrane. In every case indeed the membrane was washed with jealous care, but the fact is palpable, that it is almost impossible to divest the membrane of every trace of bitterness; when this is effected as far as practicable, in a very few minutes the damp membrane increases perceptibly in bitterness. When washed, submitted to pressure between folds of blotting-paper, stretched out on a board and dried as rapidly as possible in a current of warm air, it is still bitter. May not the bitterness alluded to in the above cases be attributed to disintegration of the mucous membrane itself? The following experiments seemed sufficiently simple in their conditions and adapted to answer the query. A body was selected in which well-known molecular disturbances are easily established—cane-sugar.

Sugar. December 28.—A portion of membrane was covered with a solution of white sugar; another portion, of the same size, was covered with lukewarm water, and both were exposed to a temperature of 32° C. One hour having elapsed, the watery solution was just perceptibly bitter, the saccharine solution decidedly so.

December 29.—The watery solution was rendered very slightly

turbid on the addition of diluted acetic acid; this reagent, however, threw down a distinct precipitate from the saccharine solution.

January 5, 1856 (from note-book).—"The mucous membrane infused in simple water is today looking disintegrated, in layers, the solution opake and slimy; slightly alkaline, just bitter. The mucous membrane in sugar very bitter, perfectly transparent. I believe that the difference of the mucous membrane, as infused in water and in syrup, appears to be well established."

January 9.—"The sugar solution is perfectly transparent, *very bitter*, very slightly alkaline; the membrane is much swelled out and thickened; *fresh*. The watery solution is becoming decomposed, alkaline, *has lost its bitter taste*, very turbid; the membrane is shrivelled and separating into layers. Microscopic examination referred the turbidness to broken-down epithelium."

The report of the above series of experiments has been thus minutely transcribed, because it seems to place the active agency of the mucous membrane beyond reasonable doubt, so far as the class of bodies alluded to is concerned; but principally because, as will be seen in the sequel, Pettenkofer's method alone appears to fail in some cases as a discriminating test of the bile.

Action of the Mucous Membrane of the Gall-bladder upon Albumen.

January 27.—"At 3 P.M. took a portion of dry mucous membrane and carefully washed it in several waters; it was then plunged into the white of an egg. 8 P.M. the glairy fluid is bitter."

January 31.—"The solution apparently increasing in bitterness; a little water added to supply the loss by evaporation."

February 18.—"The albumen solution has from time to time been diluted with water. Today I can barely detect bitterness, nor is the colour changed. On applying Pettenkofer's test, the play of colour, supposed to be characteristic of bile, was very distinct in the fluid portion; the albumen coagulated by the heat, retaining its white colour."

This result was perplexing; on the 19th, therefore, an experiment was made on the white of an egg, *per se*, to ascertain whether the effect was due to the albumen. The white of an egg was first boiled in water, to coagulate the albumen, and the filtered fluid, containing soluble albumen and probably other organic matters, was examined

by Pettenkofer's method. On adding strong sulphuric acid the fluid underwent further coagulation, and the liquid portion became of a beautiful rose colour. It remains to be determined whether the white of the egg contains any of the elements of the bile, or whether Pettenkofer's method fails, as a discriminating test of the bile, in the presence of soluble albumen.

Having thus established the fact that the mucous membrane of the gall-bladder is capable of producing changes on the bodies and under the circumstances above stated, it became an important object of inquiry whether the hepatic fluid proper is capable of being influenced by its contact; but how are we to isolate this secretion? If we take a portion of ox-liver, bruise it down, express the fluid, doubtless containing a large proportion of liver-bile, and place this fluid under an exhausted receiver over sulphuric acid, if in sufficient quantity for examination, it will be decomposed before evaporation is completed. The same fluid undergoes changes also very rapidly at a slightly elevated temperature. It was found in fact that from the temperature of 34° C. to about 60° C. putrefaction is easily produced in ox-liver, whereas, if plunged into water at the boiling-point, no considerable changes of a putrefactive nature occurred. Supposing then we plunge a very thin slice of liver into boiling water, we at once coagulate the albumen, or rather such portion of it as is insoluble in boiling water; we break up the hepatic vessels and obtain a fluid containing a considerable quantity of hepatic bile. It was found better not to keep up the boiling for any lengthened time, as the solution, in that case, contains much soluble albumen. The liquid then having been allowed to boil for a few minutes, was removed from the fire and strained through a cloth; the turbid solution cooled as rapidly as possible; the upper portion poured off from the deposit, and thus experimented upon.

Diluted acetic acid caused no precipitate, nor was any perceptible reaction produced by Pettenkofer's method, which would seem to indicate that the reaction observed in the white of the egg was not occasioned by the presence of albumen.

December 21, 1855.—As in the previous cases, a portion of well-washed mucous membrane was covered with the above solution and exposed to a temperature of 38° C., 39° C. being considered as the maximum temperature of the ox. *In half an hour, on applying*

Pettenkofer's test, the characteristic colour was beautifully developed.

December 22.—The solution last alluded to was this morning *distinctly yellow,* very bitter, and formed a precipitate with diluted acetic acid. A musky odour is also perceptible.

On the 21st of December another portion of membrane was covered with the liver-broth and left at the ordinary temperature of the room in which the operation was conducted, 10° C. After thirty hours' digestion, the fluid was in the slightest degree bitter; it was then exposed to a temperature of 50° C. Three hours having elapsed, it was again examined and found decidedly bitter.

December 23.—Cursorily examined the fluid at 4 P.M.; it is noted down as intensely bitter, becoming yellow, with slight musky odour.

December 24.—The solution, just undergoing metamorphosis on December 21st, was this day found as yellow as a diluted solution of ox-gall, musky odour distinct, intensely bitter. Another remarkable feature in common with ox-gall as it is separated from the bladder was now developed: on pouring it into a glass for precipitation with acetic acid, it was found glairy, and instead of running off like water as it did originally, the drops were viscid like a solution of gum-arabic. The bitter taste was now also converted into a sweetish-bitter, identical with the organic matter in ox-bile. The solution gave a dense precipitate with diluted acetic acid, and the peculiar reaction of Pettenkofer's test was most satisfactorily exhibited. It may not be considered unimportant to mention, that, on repeating these experiments a few days ago, a portion of the solution, treated as above, was placed in the hands of a bystander wholly ignorant of the matter, with a request to smell without looking at it; the report was, "You are mixing up some indian ink;" indeed, the odour of musk seems to be one of the most important conjunctive indications of the presence of bile, after a few hours' exposure to atmospheric air.

Many more experiments are registered in the author's rough notes; some of these have been repeated within the last few days, all with confirmatory results; the following generalizations therefore appear legitimately deduced from the research:—

1st, That the mucus of the gall-bladder is not merely a secretion destined to lubricate the interior of that organ and protect it from

the irritation of its other contents, but is an essential integral portion of the cystic bile.

2ndly, That the gall-bladder is not merely a receptacle and reservoir for the bile, but an organ highly endowed with organic functions; and that the proper secretion of the liver is converted into cystic bile mainly through the agency of its mucous membrane.

In thus breaking up the surface of an interesting field of research, the writer is fully aware that a great amount of labour must still be expended upon its development; he would also be understood to regard these experiments merely as expressing the results of non-vital reactions. We can hardly indeed doubt that, under the influence of vitality, acting through the medium of that most important department of the nervous system, the *solar plexus*, molecular changes, not improbably analogous to or identical with those which we have described, may be carried on with an energy and efficiency which we cannot hope to witness in the laboratory. Professor Clark, of Cambridge, has already suggested the extension of the research to the case of animals which have no gall-bladder, and in which the hepatic secretion is at once poured into the duodenum, to take its part in the process of assimilation. The conjecture may not be far from the truth, that the mucous lining of the intestinal canal, the parotid gland, the pancreas, the kidney, the urinary bladder, has each its specific predestined function to perform; and that in working out the subject, we may fall upon many a useful fact, many a beautiful analogy, and much to supply the wants and alleviate the sufferings of man.

Thursday the 29th of May having been set apart for the celebration of the Peace, the President announced that the next ordinary meeting of the Society would be held on Thursday, the 12th of June,

June 3, 1856.

The LORD WROTTESLEY, President, in the Chair.

A Special General Meeting was held this day, to consider a proposal from Her Majesty's Government to give apartments to the Society in Burlington House, contained in the following letter:—

Treasury Chambers, 22nd May, 1856.

MY LORD,—I am directed by the Lords Commissioners of Her Majesty's Treasury to acquaint your Lordship, with reference to the views set forth in your Letter to the Duke of Argyll of the 30th ult., which has been laid before this Board, that Her Majesty's Government are not at present in a position to enable them to state any definite views with respect to the project for the juxtaposition of the principal Scientific Societies in a building to be erected in a convenient and central locality.

I have to state that their Lordships are however prepared so far to concede to the views advanced by your Lordship on behalf of a large number of persons connected with science, as to allow the temporary location of the Linnean and Chemical Societies, in conjunction with the Royal Society, in the present building of Burlington House, on the following conditions, viz.—

1. That the removal of the Royal Society from Somerset House shall not prejudice the position of the other Societies located in that building, in regard to the terms on which they are permitted to occupy their present apartments.

2. That the Royal Society shall be put in possession of the main building of Burlington House, on the understanding that they will, in communication with the Linnean and Chemical Societies, assign suitable accommodation therein for those bodies.

3. A common Library to be formed for the use of the three Societies, on the understanding that suitable arrangements shall be made

for the admission thereto, for purposes of reference and study, of men of letters and science, on orders given by Fellows of the three Societies*.

4. The Societies to be allowed the use of the Hall which it is proposed to construct in the West Wing of Burlington House, at such times as it may not be required by the Senate of the University of London, it being distinctly understood that this permission is to be so exercised as not in any way to interfere with the convenience of the University.

5. The Collection of Portraits belonging to the Royal Society to be hung on the walls of the proposed Hall, and to be open to the inspection of the public under such regulations as may be convenient, and subject especially to the proviso in the preceding clause.

6. That the adoption of this temporary arrangement shall not in any respect be held to weaken the claim of the Royal Society to permanent accommodation.

I have the honour, &c.,

(Signed) JAMES WILSON.

To the President of the Royal Society.

The President having stated to the Meeting the circumstances under which the offer of Burlington House had been made to the Royal Society by Her Majesty's Government, Sir Benjamin Brodie, Bart., V.P., proposed the following Resolution :—

"That the Council be authorized to accept and carry out the proposal of the Government as to the occupation of Burlington House, on the understanding that the Hall, which it is proposed to construct in the West Wing, and which is to contain the Portraits belonging to the Royal Society, shall be placed in the custody of the Royal

* The President stated, that he had intimated to the Secretary of the Treasury that, in his opinion, the Council would understand the third condition in the foregoing letter as implying the mutual access to the three libraries by the Fellows of the three Societies for the purposes of reference and study, but not as altering in any respect the ownership or custody of the several Libraries; and that in future, as heretofore, the loan of the books of any of the three libraries should be confined to the Fellows of that Society to which they belong. The President further stated, that Mr. Wilson assented to the above explanation of the minute, so far as it relates to the formation of a common library.

Society, subject to the free use of it by the Senate of the University of London at all times at which it may be required for their Examinations and Public Meetings."

The Resolution having been seconded by Mr. Bell, was put from the Chair, and carried. The Society then adjourned.

June 5, 1856.

The LORD WROTTESLEY. President, in the Chair.

The Annual General Meeting for the Election of Fellows was held this day.

Sir George Back, Capt. R.N., and John Gwynn Jeffreys, Esq., were, with the consent of the Meeting, appointed Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present having been collected, the following gentlemen were declared duly elected :—

John Hutton Balfour, M.D.
Edward W. Binney, Esq.
Sir John Bowring.
Sir John Fox Burgoyne, Bart.
Philip Henry Gosse, Esq.
Robert Harkness, Esq.
Cæsar Henry Hawkins, Esq.
Manuel John Johnson, Esq.
John Carrick Moore, Esq.
Henry Minchin Noad, Esq.
Edmund Potter, Esq.
Rev. T. Romney Robinson, D.D.
Henry Hyde Salter, M.D.
Archibald Smith, Esq.
Capt. Thomas A. B. Spratt, R.N.

On the motion of Dr. Miller, seconded by Dr. Stenhouse, the thanks of the Meeting were given to the Scrutators, and the Society then adjourned.

June 12, 1856.

The LORD WROTTESLEY, President, in the Chair.

The following gentlemen were admitted into the Society :—

Sir John Fox Burgoyne, Bart.
Philip Henry Gosse, Esq.
Archibald Smith, Esq.

The following communications were read :—

I. "On the Construction of the Imperial Standard Pound, and its copies of Platinum; and on the comparison of the Imperial Standard Pound with the Kilogramme des Archives." By W. H. Miller, M.A., F.R.S., Professor of Mineralogy in the University of Cambridge.—Part II. Received June 7, 1856.

(Abstract.)

The Quartz Weight

The hardness of quartz, its capability of taking a high polish, the absence of any hygroscopic properties, and its indestructibility at the ordinary temperature of the atmosphere by any chemical agent except hydrofluoric acid, are such valuable qualities in a substance used for the construction of weights, that Professor Steinheil adopted it as the material for a copy of the kilogramme. The only objection to the use of a weight made of quartz is, that on account of the large amount of air displaced, the barometer and thermometer must be observed with extreme care during its comparison with a weight made of any ordinary metal. The Committee commissioned Mr. Barrow to construct a weight of quartz sufficiently near to 7000 grs. to admit of readily deducing the pound from it. Its form is that of a cube of about 2·2 inches, having its edges and angles rounded.

Its apparent weight in air is intermediate between that of a pound of platinum and a pound of brass, approaching more nearly to the latter than to the former.

Six series of weighings in water gave for the absolute weights of water displaced by it at 18° C.—

	2639·831
	2639·809
	2639·838
	2639·825
	2639·819
	2639·814
Mean	2639·823

Whence $$\frac{\text{density quartz at } 18^\circ \text{ C.}}{\text{density water at } 18^\circ \text{ C.}} = 2{\cdot}652590.$$

Denoting the quartz weight by Q, and the new Imperial Standard Pound by I, the comparisons of Q with I in air, reduced to a vacuum, gave

	Grains.		No. of Obs.
	$Q = I + 2{\cdot}36801$	......	40
	$Q = I + 2{\cdot}36871$	......	40
	$Q = I + 2{\cdot}36817$	......	40
	$Q = I + 2{\cdot}36782$	......	40
	$Q = I + 2{\cdot}36715$	......	40
Mean	$Q = I + 2{\cdot}36797$	......	200

Secondary Standards.

Thirty secondary standards, of gun-metal protected by amalgam-gilding, were constructed by Mr. Barrow. The densities of these were determined by weighing them in air and in water, and their absolute weights by comparison either with I or with T+D.

Values of the densities at the freezing-point in terms of the maximum density of water, absolute weights in terms of I, and apparent weights, at Somerset House in air of the temperature

65·66 Fahrenheit, under the pressure of 29·75 inches of mercury at the freezing-point of water ($t = 18^\circ{\cdot}7$ C., $b = 755^{\mathrm{mm}}{\cdot}64$), or in air for which $10 + \log \Delta = 7{\cdot}07835$, in terms of the commercial pound W of the same density as the lost standard troy pound.

No.	Density.	Absolute values.	Commercial values.
		grain.	grain.
1	8·3613	I−0·00732	W+0·01963
2	8·3416	I−0·03582	W−0·01135
3	8·3046	I+0·00510	W+0·02510
4	8·3650	I+0 00425	W+0·03154
5	8·0612	I+0·01783	W+0·00730
6	8·2878	I−0·01714	W+0·00080
7	8·1216	I+0·01933	W+0·01654
8	8·1632	I+0·01428	W+0·01679
9	7·3761	I+0·11611	W+0·00422
10	8·2838	I−0·03910	W−0·02165
11	8·3630	I−0·04208	W−0·01503
12	8·3192	I−0·02060	W+0·00115
13	8·4318	I−0·03331	W+0·00191
14	8·3496	I−0·02844	W−0·00301
15	8·3611	I−0·02022	W+0·00667
16	8·0735	I−0·02747	W−0·03640
17*a*	8·1172	I−0·02614	W−0·02948
17*b*	8·5589	I−0·04428	W+0·00542
18	8·3037	I−0·00129	W+0·01857
19	8·3397	I−0·01473	W+0·00950
21	7·9737	I+0·03971	W+0·01777
22	8·1986	I−0·01214	W−0·00523
23	8·1514	I+0·01557	W+0·01655
24	8·1429	I−0·03932	W−0·03941
25	8·1016	I+0·00180	W−0·00354
26	8·1522	I−0·00112	W−0·00001
27	8·1619	I+0·01405	W+0·01635
28	8·1260	I−0·00416	W−0·00638
29	8·1845	I−0·00222	W+0·00293
30	8·1529	I−0·00170	W−0·00050

II. "On the Determination of Unknown Functions which are involved under Definite Integrals." By J. GOMES DE SOUZA, Professor of Mathematics in the Military Academy of Rio Janeiro. Communicated by Professor STOKES, Sec. R.S.

The author, after referring to a previous memoir on the same subject, presented by him to the French Academy, proposes to himself

the problem of determining the function ϕ which (f, F being given functions, and the limits α, β of the integration being also given) satisfies the equation

$$\int_{\alpha}^{\beta} f(x,\theta)\ \phi(x+\theta)\ d\theta = \mathrm{F}x.$$

He observes, that, unlike the methods employed in his former memoir, and the solutions there employed, which are quite rigorous, the methods of the present memoir depend upon developments into series, the strictness of which has been contested by some mathematicians; but that passing over these difficulties, he has solved the famous problem, the solution of which has been vainly sought after for the last two hundred years, because on the above-mentioned equation depends the integration of the generally linear equation of any order whatever of two variables, and consequently the whole Integral Calculus. The solution first obtained by the author, and which he afterwards exhibits under a variety of different forms, is as follows:—

Theorem I.—The equation being given,

$$\int_{\alpha}^{\beta} f(x,\theta)\,\phi(x+\theta)\,d\theta = \frac{\mathrm{F}_2(x)}{\mathrm{F}_1(x)} = \mathrm{F}(x),$$

where $f(x,\theta)$ is a given function of x and θ; $\mathrm{F}_2(x)$ is a given function of x such that the equation $\mathrm{F}_2(x)=\infty$ cannot hold good for any finite value of x; $\mathrm{F}_1(x)$ a given function of x containing all the factors which render $\mathrm{F}(x)$ infinite, and the function $\mathrm{F}(x)$ being absolutely arbitrary; and α and β being given constants (independent therefore of x and θ), the expression for ϕx which satisfies the preceding equation is

$$\phi x = \frac{\mathrm{F}_2(a_1)}{f_1(a_1)\mathrm{F}_1{}'(a_1)}\, e^{x\Phi(a_1)} + \frac{\mathrm{F}_2(a_2)}{f_2(a_2)\mathrm{F}'_1(a_2)}\, e^{x\Phi(a_2)} + \&c.,$$

where $f_r(x)$ is determined by

$$f_r(x) = (x-a_r)\, e^{x\Phi(a_r)} \int_{\alpha}^{\beta} f(x,\theta)\, e^{m_r\theta} d\theta.$$

$\Phi(a_r)$ is a root of the equation

$$\frac{1}{\int_{\alpha}^{\beta} e^{m_r\theta} f(a_r,\theta)\, d\theta} = 0$$

solved relatively to m_r, and a_1, a_2, a_3, &c. are the roots of

$$F_1(x) = 0.$$

The author afterwards considers the equation

$$\int_\alpha^\beta f(x, \theta)\,\phi(\theta)\,d\theta = F(x),$$

and the solution of a linear equation is at once made to depend upon this as follows: viz. given for the determination of the function $\phi\,x$ the equation

$$f(x, 0)\,\phi(x) + f(x, 1)\,\frac{d\phi(x)}{dx} + \text{\&c.} = F(x).$$

Assume

$$\phi(x) = \int_\alpha^\beta e^{\theta x}\,\psi(\theta)\,.\,d\theta,$$

α, β being constants, and $\psi(\theta)$ a function of θ to be determined. It is always permitted to assume this equation.

By this means, writing for shortness

$$f_1(x, \theta) = f(x, 0) + f(x, 1)\,\theta + \text{\&c.},$$

the equation becomes

$$\int_\alpha^\beta e^{\theta x} f_1(x, \theta)\,\psi(\theta)\,d\theta = F(x),$$

which is of the desired form.

A solution which occurred to the author after the memoir was drawn up, is as follows: viz. given, as before, the equation

$$\int_\alpha^\beta f(x, \theta)\,\phi(x+\theta)\,d\theta = F(x),$$

then $\Phi(\omega\sqrt{-1}$ being determined by the equation

$$\frac{1}{\int_\alpha^\beta e^{\theta\Phi(\omega\sqrt{-1})} f(\omega\sqrt{-1}, \theta)\,d\theta} = 0,)$$

and putting, for abbreviation,

$$e^{x(\Phi\omega\sqrt{-1})}\,(x - \omega\sqrt{-1}) \int_\alpha^\beta e^{\theta\Phi(\omega\sqrt{-1})} f(x, \theta)\,d\theta = f(x, \omega\sqrt{-1}),$$

the equation

$$\varphi x = \frac{1}{2\pi} \int_{-\infty}^{\infty} \frac{e^{x\Phi(\omega\sqrt{-1})}\,\mathrm{F}(\omega\sqrt{-1})}{f(\omega\sqrt{-1}, \omega\sqrt{-1})}\, d\omega$$

gives the solution of the problem.

The above-mentioned formulæ are selected out of a great number of very general results contained in the memoir.

III. Letter from Dr. W. Bird Herapath to Professor Stokes, "On the Detection of Strychnia by the formation of Iodo-strychnia." Communicated by Professor Stokes, Sec. R.S. Received June 12, 1856.

Bristol, June 7, 1856.

My dear Sir,—Will you do me the favour to announce to the Royal Society, that I have been engaged during some time past in the application of my discovery of the optical properties of iodo-strychnia to the detection of this alkaloid in medico-legal inquiries? I find it is perfectly possible to recognize the 10,000th part of a grain of strychnia in pure solutions by this method, even when experimenting on very minute quantities. In one experiment I took $\frac{1}{1000}$th of a grain only, and having produced ten crystals of nearly equal size, of course each one, possessing distinct and decided optical properties, could not represent *more* than the $\frac{1}{10000}$th part of a grain; in fact, it really represents much less, inasmuch as one portion of the strychnia is converted by substitution into a soluble hydriodate, and of course remains dissolved in the liquid.

I had hoped to have been able to complete this matter during this summer, but I now find it impossible to do so in time for this session of the Royal Society. I trust to be able to do so before Christmas, however. Will you oblige me by getting this notice inserted in the 'Proceedings,' as a new test for strychnia at this juncture possesses considerable interest, the colour-tests having been so dubiously spoken of recently by toxicologists?

In order to operate in this experiment, it is merely necessary to use diluted spirit of wine, about in the proportions of one part of spirit

to three of water, as the solvent medium, and to employ the smallest possible quantity of the tincture of iodine as the reagent, and after applying heat for a short time, to set in repose. On spontaneous evaporation or cooling, the optical crystals deposit themselves, and may be recognized by the polarizing microscope, according to the description given of this substance in a former notice to the Society in June last.

You may remember that this proposition was also contained in my paper on iodo-strychnia, which was withdrawn from the Royal Society by me in June last in consequence of a necessity for revision and the completion of experiments requisite to settle the formula of that peculiar substance, and the introduction of an abstract of the literature concerning it.

I remain, &c.,
W. Bird Herapath.

IV. "Dynamical Illustrations of the Magnetic and the Heliçoidal Rotatory Effects of Transparent Bodies on Polarized Light." By Professor W. Thomson, F.R.S. Received May 10, 1856.

The elastic reaction of a homogeneously strained solid has a character essentially devoid of all heliçoidal and of all dipolar asymmetry. Hence the rotation of the plane of polarization of light passing through bodies which either intrinsically possess the heliçoidal property (syrup, oil of turpentine, quartz crystals, &c.), or have the magnetic property induced in them, must be due to elastic reactions dependent on the heterogeneousness of the strain through the space of a wave, or to some heterogeneousness of the luminous motions* dependent on a heterogeneousness of parts of the matter of lineal dimensions not infinitely small in comparison with the wave length. An infinitely homogeneous solid could not possess either of those

* As would be were there different sets of vibrating particles, or were Rankine's important hypothesis true, that the vibrations of luminiferous particles are directly affected by pressure of a surrounding medium in virtue of its inertia.

properties if the stress at any point of it was influenced only by parts of the body touching it; but if the stress at one point is directly influenced by the strain in parts at distances from it finite in comparison with the wave length, the heliçoidal property might exist, and the rotation of the plane of polarization, such as is observed in many liquids and in quartz crystals, could be explained as a direct dynamical consequence of the statical elastic reaction called into play by such a strain as exists in a wave of polarized light. It may, however, be considered more probable that the matter of transparent bodies is really heterogeneous from one part to another of lineal dimensions not infinitely small in comparison with a wave length, than that it is infinitely homogeneous and has the property of exerting finite direct "molecular" force at distances comparable with the wave length: and it is certain that any spiral heterogeneousness of a vibrating medium must, if either right-handed or left-handed spirals predominate, cause a finite rotation of the plane of polarization of all waves of which lengths are not infinitely great multiples of the steps of the structural spirals. Thus a liquid filled homogeneously with spiral fibres, or a solid with spiral passages through it of steps not less than the forty-millionth of an inch, or a crystal with a right-handed or a left-handed geometrical arrangement of parts of some such lineal dimensions as the forty-millionth of an inch, might be certainly expected to cause either a right-handed or a left-handed rotation of ordinary light (the wave length being $\frac{1}{40,000}$th of an inch for homogeneous yellow).

But the magnetic influence on light discovered by Faraday depends on the direction of motion of moving particles. For instance, in a medium possessing it, particles in a straight line parallel to the lines of magnetic force, displaced to a helix round this line as axis, and then projected tangentially with such velocities as to describe circles, will have different velocities according as their motions are round in one direction (the same as the nominal direction of the galvanic current in the magnetizing coil), or in the contrary direction. But the elastic reaction of the medium must be the same for the same displacements, whatever be the velocities and directions of the particles; that is to say, the forces which are balanced by centrifugal force of the circular motions are equal, while the luminiferous motions are unequal. The absolute circular motions being therefore either

equal or such as to transmit equal centrifugal forces to the particles initially considered, it follows that the luminiferous motions are only components of the whole motion; and that a less luminiferous component in one direction, compounded with a motion existing in the medium when transmitting no light, gives an equal resultant to that of a greater luminiferous motion in the contrary direction compounded with the same non-luminous motion. I think it is not only impossible to conceive any other than this dynamical explanation of the fact that circularly polarized light transmitted through magnetized glass parallel to the lines of magnetizing force, with the same quality, right-handed always, or left-handed always, is propagated at different rates according as its course is in the direction or is contrary to the direction in which a north magnetic pole is drawn; but I believe it can be demonstrated that no other explanation of that fact is possible. Hence it appears that Faraday's optical discovery affords a demonstration of the reality of Ampère's explanation of the ultimate nature of magnetism; and gives a definition of magnetization in the dynamical theory of heat. The introduction of the principle of moments of momenta ("the conservation of areas") into the mechanical treatment of Mr. Rankine's hypothesis of "molecular vortices," appears to indicate a line perpendicular to the plane of resultant rotatory momentum ("the invariable plane") of the thermal motions as the magnetic axis of a magnetized body, and suggests the resultant moment of momenta of these motions as the definite measure of the "magnetic moment." The explanation of all phenomena of electro-magnetic attraction or repulsion, and of electro-magnetic induction, is to be looked for simply in the inertia and pressure of the matter of which the motions constitute heat. Whether this matter is or is not electricity, whether it is a continuous fluid interpermeating the spaces between molecular nuclei, or is itself molecularly grouped; or whether all matter is continuous, and molecular heterogeneousness consists in finite vortical or other relative motions of contiguous parts of a body; it is impossible to decide, and perhaps in vain to speculate, in the present state of science.

I append the solution of a dynamical problem for the sake of the illustrations it suggests for the two kinds of effect on the plane of polarization referred to above.

Let the two ends of a cord of any length be attached to two

points at the ends of a horizontal arm made to rotate round a vertical axis through its middle point at a constant angular velocity, ω, and let a second cord bearing a weight be attached to the middle of the first cord. The two cords being each perfectly light and flexible, and the weight a material point, it is required to determine its motion when infinitely little disturbed from its position of equilibrium.*

Let l be the length of the second cord, and m the distance from the weight to the middle point of the arm bearing the first. Let x and y be, at any time t, the rectangular coordinates of the position of the weight, referred to the position of equilibrium O, and two rectangular lines OX, OY, revolving uniformly in a horizontal plane in the same direction, and with the same angular velocity as the bearing arm; then, if we choose OX parallel to this arm, and if the rotation be in the direction with OY preceding OX, we have, for the equations of motion,

$$\frac{d^2x}{dt^2}-\omega^2x-2\omega\frac{dy}{dt}=-\frac{g}{l}x,$$

$$\frac{d^2y}{dt^2}-\omega^2y+2\omega\frac{dx}{dt}=-\frac{g}{m}y.$$

If for brevity we assume

$$\frac{1}{2}\left(\frac{g}{l}+\frac{g}{m}\right)=n^2, \text{ and } \frac{1}{2}\left(\frac{g}{l}-\frac{g}{m}\right)=\lambda^2,$$

we find, by the usual methods, the following solution :—

$$x=\text{A}\cos\{[\omega^2+n^2+(\lambda^4+4n^2\omega^2)^{\frac{1}{2}}]^{\frac{1}{2}}t+\alpha\}$$
$$+\text{B}\cos\{[\omega^2+n^2-(\lambda^4+4n^2\omega^2)^{\frac{1}{2}}]^{\frac{1}{2}}t+\beta\},$$

$$y=-\frac{2\omega^2-\lambda^2+(\lambda^4+4n^2\omega^2)^{\frac{1}{2}}}{2\omega[\omega^2+n^2+(\lambda^4+4n^2\omega^2)^{\frac{1}{2}}]^{\frac{1}{2}}}\text{A}\sin\phi-\frac{2\omega^2-\lambda^2-(\lambda^4+4n^2\omega^2)^{\frac{1}{2}}}{2\omega[\omega^2+n^2-(\lambda^4+4n^2\omega^2)^{\frac{1}{2}}]^{\frac{1}{2}}}\text{B}\sin\psi,$$

where A, α, B, β are arbitrary constants, and ϕ and ψ are used for brevity to denote the arguments of the cosines appearing in the expression for x.

The interpretation of this solution, when ω is taken equal to the component of the earth's angular velocity round a vertical at the

* By means of this arrangement, but without the rotation of the bearing arm, a very beautiful experiment, due to Professor Blackburn, may be made by attaching to the weight a bag of sand discharging its contents through a fine aperture.

locality, affords a full explanation of curious phenomena which have been observed by many in failing to repeat Foucault's admirable pendulum experiment. When the mode of suspension is perfect, we have $\lambda=0$; but in many attempts to obtain Foucault's result, there has been an asymmetry in the mode of attachment of the head of the cord or wire used, or there has been a slight lateral unsteadiness in the bearings of the point of suspension, which has made the observed motion be the same as that expressed by the preceding solution, where λ has some small value either greater than or less than ω, and n has the value $\sqrt{\frac{g}{l}}$. The only case, however, that need be considered as illustrative of the subject of the present communication is that in which ω is very great in comparison with n. To obtain a form of solution readily interpreted in this case, let

$$[\omega^2+n^2+(\lambda^4+4n^2\omega^2)^{\frac{1}{2}}]^{\frac{1}{2}}=\omega+\rho, \qquad [\omega^2+n^2-(\lambda^4+4n^2\omega^2)^{\frac{1}{2}}]^{\frac{1}{2}}=\omega-\sigma,$$

$$\frac{2\omega^2-\lambda^2+(\lambda^4+4n^2\omega^2)^{\frac{1}{2}}}{2\omega[\omega^2+n^2+(\lambda^4+4n^2\omega^2)^{\frac{1}{2}}]^{\frac{1}{2}}}=1+e, \qquad \frac{2\omega^2-\lambda^2-(\lambda^4+4n^2\omega^2)^{\frac{1}{2}}}{2\omega[\omega^2+n^2-(\lambda^4+4n^2\omega^2)^{\frac{1}{2}}]^{\frac{1}{2}}}=1-f.$$

The preceding solution becomes

$$\begin{aligned} x&=\mathrm{A}\cos\{(\omega+\rho)t+\alpha\}+\mathrm{B}\cos\{(\omega-\sigma)t+\beta\}\\ y&=-\mathrm{A}\sin\{(\omega+\rho)t+\alpha\}-\mathrm{B}\sin\{(\omega-\sigma)t+\beta\}\\ &\quad -e\mathrm{A}\sin\{(\omega+\rho)t+\alpha\}+f\mathrm{B}\sin\{(\omega-\sigma)t+\beta\}. \end{aligned}$$

To express the result in terms of coordinates ξ, η, with reference to fixed axes, instead of the revolving axes OX, OY, we may assume

$$\xi=x\cos\omega t-y\sin\omega t, \qquad \eta=x\sin\omega t+y\cos\omega t.$$

Then we have

$$\begin{aligned} \xi&=\mathrm{A}\cos(\rho t+\alpha)+\mathrm{B}\cos(\sigma t-\beta)\\ &\quad+\big(e\mathrm{A}\sin\{(\omega+\rho)t+\alpha\}-f\mathrm{B}\sin\{(\omega-\sigma)t+\beta\}\big)\sin\omega t\\ \eta&=-\mathrm{A}\sin(\sigma t+\alpha)+\mathrm{B}\sin(\sigma t-\beta)\\ &\quad+\big(-e\mathrm{A}\sin\{(\omega+\rho)t+\alpha\}+f\mathrm{B}\sin\{(\omega-\sigma)t+\beta\}\big)\cos\omega t. \end{aligned}$$

When ω is very large, e and f are both very small, and the last two terms of each of these equations become very small periodic terms, of very rapidly recurring periods, indicating a slight tremor in the resultant motion. Neglecting this, and taking $\alpha=0$ and $\beta=0$, as we may do without loss of generality, by properly choosing the axes

of reference, and the era of reckoning for the time, we have finally, for an approximate solution of a suitable kind,

$$\xi = \text{A} \cos \rho t + \text{B} \cos \sigma t,$$
$$\eta = -\text{A} \sin \rho t + \text{B} \sin \sigma t.$$

The terms B, in this expression, represent a circular motion of period $\frac{2\pi}{\sigma}$, in the positive direction (that is, from the positive axis of ξ to the positive axis of η), or in the same direction as that of the rotation ω; and the terms A represent a circular motion, of period $\frac{2\pi}{\rho}$, in the contrary direction. Now, ω being very great, ρ and σ are very nearly equal to one another; but ρ is rather less than σ, as the following approximate expressions derived from their exact values expressed above, show:—

$$\rho = n + \frac{1}{8}\frac{\lambda^4}{\omega^2 n} - \frac{1}{8}\frac{\lambda^4}{\omega^3}, \qquad \sigma = n + \frac{1}{8}\frac{\lambda^4}{\omega^2 n} + \frac{1}{8}\frac{\lambda^4}{\omega^3}.$$

Hence the form of solution simply expresses that circular vibrations of the pendulum in the contrary directions have slightly different periods, the shorter, $\frac{2\pi}{\sigma}$, when the motion of the pendulum follows that of the arm supporting it, and the longer, $\frac{2\pi}{\rho}$, when it is in the contrary direction. The equivalent statement, *that if the pendulum be simply drawn aside from its position of equilibrium, and let go without initial velocity, the vertical plane of its motion will rotate slowly at the angular rate* $\frac{1}{2}(\sigma - \rho)$, is expressed most shortly by taking A = B, and reducing the preceding solution to the form

$$\xi = 2\text{A} \cos \varpi t \cos n't,$$
$$\eta = 2\text{A} \sin \varpi t \cos n't,$$

where

$$n' = \frac{1}{2}(\sigma + \rho), \text{ or, approximately, } n' = n + \frac{1}{8}\frac{\lambda^4}{\omega^2 n},$$

and

$$\varpi = \frac{1}{2}(\sigma - \rho), \text{ or, approximately, } \varpi = \frac{1}{8}\frac{\lambda^4}{\omega^3}.$$

It is a curious part of the conclusion thus expressed, that the faster the bearing arm is carried round, the slower does the plane of a simple vibration of the pendulum follow it. When the bearing arm is carried round infinitely fast, the plane of a vibration of the

pendulum will remain steady, and the period will be n; in other words, the motion of the pendulum will be the same as that of a simple pendulum whose length is $\frac{2}{\frac{1}{e}+\frac{1}{m}}$, or a harmonic mean between the effective lengths in the two principal planes of the actual pendulum.

It is easy to prove from this, that if a long straight rod, or a stretched cord possessing some rigidity, unequally elastic or of unequal dimensions, in different transverse directions, be made to rotate very rapidly round its axis, and if vibrations be maintained in a line at right angles to it through any point, there will result, running along the rod or cord, waves of sensibly rectilineal transverse vibrations, in a plane which in the forward progress of the wave, turns at a uniform rate in the same direction as the rotation of the substance; and that if $\frac{2\pi}{\omega}$ be the period of rotation of the substance, and l and m the lengths of simple pendulums respectively isochronous with the vibrations of two plane waves of the same length, a, in the planes of maximum and of minimum elasticity of the substance, when destitute of rotation, the period of vibration in a wave of the same length in the substance when made to rotate will be

$$\frac{2\pi}{n\left(1+\frac{1}{8}\frac{\lambda^4}{\omega^2 n^2}\right)};$$

and the angle through which the plane of vibration turns, in the propagation through a wave length, will be

$$\frac{\pi}{4}\frac{\lambda^4}{n\omega^3};$$

or the number of wave lengths through which the wave is propagated before its plane turns once round, will be

$$\frac{8n\omega^3}{\lambda^4};$$

where, as before,

$$n=\sqrt{g\cdot\frac{1}{2}\left(\frac{1}{l}+\frac{1}{m}\right)},\qquad \lambda=\sqrt{g\cdot\frac{1}{2}\left(\frac{1}{l}-\frac{1}{m}\right)},$$

and ω denotes the angular velocity with which the substance is made to rotate.

If next we suppose the rod or cord to be slightly twisted about its axis, so that its directions of maximum and minimum elasticity shall lie on two rectangular heliçoidal surfaces (*heliçoïdes gauches*), and if, while regular rectilineal vibrations are maintained at one point of it with a period to which the wave length corresponding is a very large multiple of the step of the screw, the substance be made to rotate so rapidly as to make the velocity of a point carried along one of the screw surfaces in a line parallel to the axis be equal to the velocity of propagation of a wave, it is clear that a series of sensibly plane waves will run along the rod or cord with no rotation of the plane of vibration. The period of vibration of a particle will be, approximately, the same as before, that is, approximately, equal to $\frac{2\pi}{n}$. Its velocity of propagation will therefore be $\frac{na}{2\pi}$, and, if s be the step of the screws, the period of rotation of the substance, to fulfil the stated condition, must be $\frac{2\pi s}{na}$, or its angular velocity $\frac{na}{s}$. Now it is easily seen that the effects of the rapid rotation, and the effects of the slight twist, may be considered as independently superimposed; and therefore the effect of the twist, with no rotation of the substance, must be to give a rotation to the plane of vibration equal and contrary to that which the rotation of the substance would give if there were no twist. But the effect on the plane of vibration, due to an angular velocity ω, of rotation of the substance, is, as we have seen, one turn in $\frac{8n\omega^3}{\lambda^4}$ wave lengths; and therefore it is one turn in $8\frac{n^4}{\lambda^4}\frac{a^3}{s^3}$ wave lengths when the angular velocity is $\frac{na}{s}$. Hence the effect of a twist amounting to one turn in a length, s, a small fraction of the wave length, is to cause the plane of vibration of a wave to turn round with the forward propagation of the wave, at the rate of one turn in $8\,\frac{n^4}{\lambda^4}\,\frac{a^3}{s^3}$ wave lengths, in the same direction as that of a point kept on one of the screw surfaces.

From these illustrations it is easy to see in an infinite variety of ways how to make structures, homogeneous when considered on a large enough scale, which (1) with certain rotatory motions of component parts having, in portions large enough to be sensibly homogeneous, resultant axes of momenta arranged like lines of magnetic

force, *shall have the dynamical property by which the optical phenomena of transparent bodies in the magnetic field are explained*; (2) with spiral arrangements of component parts, having axes all ranged parallel to a fixed line, *shall have the axial rotatory property corresponding to that of quartz crystal*; and (3) with spiral arrangements of component groups, having axes totally unarranged, *shall have the isotropic rotatory property possessed by solutions of sugar and tartaric acid, by oil of turpentine, and many other liquids.*

V. "Researches on the Action of Sulphuric Acid upon the Amides and Nitriles, with Remarks on the Conjugate Sulpho-acids." By George B. Buckton, Esq., F.L.S., F.C.S., and A. W. Hofmann, Ph.D., F.R.S. Received May 13, 1856.

(Abstract.)

Since we had the honour of addressing the Royal Society upon the subject of the behaviour of acetamide and acetonitrile towards sulphuric acid, we have completed our experiments upon the amides and nitriles, and extended our researches to other groups of bodies. The results of these additional inquiries we now beg to present in the form of a second short summary, the analytical details and the more extended description of the new compounds being given in the complete memoir, which, at the same time, we have the honour of submitting to the Society.

Before proceeding, however, to give an account of our new compounds, it may be desirable to state that several considerations, suggested by the progress of our inquiry, have induced us finally to adopt the name of Disulphometholic acid instead of the provisional term Tetrasulphomethylic acid under which we have described, in our first communication, the new acid generated by the action of sulphuric acid upon acetamide and acetonitrile.

Ethyl-Series.

Action of Sulphuric Acid upon Propionitrile.

Considerable difficulty is experienced in preparing this nitrile in a

state of purity. It was finally obtained by acting upon propionamide with anhydrous phosphoric acid.

When three parts by measure of the nitrile are cautiously mixed with two parts of fuming sulphuric acid, and heat is applied, the liquids enter into a sort of ebullition, carbonic acid being copiously evolved; at the same time a portion of propionic acid passes into the receiver, the amount of which may be lessened by raising the temperature only gradually.

At the close of the operation a tenacious mass is found in the retort, which, when dissolved in water and neutralized with carbonate of barium, furnishes two rather soluble but readily crystallizable salts, very difficult to separate one from the other. Their isolation may be conveniently effected, by converting them into the corresponding ammoniacal compounds, by precipitating their solution with carbonate of ammonium.

The filtrate yields two substances, one of which crystallizes, while the other is quite uncrystallizable.

The latter substance, when long digested with carbonate of barium, produces crystals of a barium-salt whose analysis gives numbers leading to the formula $C_6 (H_4 Ba_2) S_2 O_{10}$.

This substance is obviously sulphopropionate of barium, the compound next in series to the sulphacetate discovered by M. Melsens.

It is generally deposited from its solution in fine silky crystals, which arrange themselves in spherical groups. They are very stable, and bear a high temperature without decomposition.

The salt associated with the uncrystallizable sulphopropionate of ammonium crystallizes with ease, either in rectangular prisms or in octohedra. Similarly converted into a barium-compound, it was found by analysis to contain at 100° C.,

$$C_4 (H_4 Ba_2) S_4 O_{12} + 2HO.$$

It forms regular six-sided plates, which are moderately soluble in water, but insoluble in alcohol and in ether. It loses two equivalents of water of crystallization between 100° and 170°, but a few degrees above this temperature it is decomposed with blackening, yielding water, sulphurous acid, volatile organic products, and sulphate of barium.

We designate this salt as disulphetholate of barium.

Disulphetholic acid is prepared by precipitating a solution of the barium-salt with sulphuric acid, the excess of which is again removed by digestion with oxide of lead, and subsequent treatment with sulphuretted hydrogen. It is a crystalline and stable compound, very acid to the taste, and very deliquescent. With oxide of lead, or with carbonate of silver, it readily forms the respective salts, both of which are crystalline.

After what has been said with reference to the action of sulphuric acid upon acetamide, it is scarcely necessary to remark that the sulphopropionates and disulphetholates may be prepared with equal, or even greater facility from propionamide. Care, however, should be taken to use the amide in a perfectly dry state, which prevents in great measure the formation of free propionic acid.

Propyl-Series.

Action of Sulphuric Acid upon Butyramide.

Equal parts by volume of melted butyramide and Nordhausen sulphuric acid evolve much heat when mixed together. In the reaction two acids are eliminated, showing that the series bears a strict analogy with the deportment exhibited by the preceding group.

As the ammonia-salts of these acids are wholly uncrystallizable, their separation is almost impossible. The barium-compounds also are scarcely to be obtained of a definite form, so that it is a matter of great difficulty to procure salts of sufficient purity for exact estimation. Recourse was had to fractional precipitation by alcohol.

The first salt which was deposited formed minute grains, which adhered strongly to the sides of the glass vessel containing the solution. It gave a percentage of barium which unmistakeably indicated the formula

$$C_8\,(H_6\,Ba_2)\,S_2\,O_{10},$$

which is that of sulphobutyrate of barium.

This substance in its reactions closely resembles (with the exception of its greater solubility in water) the corresponding body of the ethyl-series. It burns like tinder, with evolution of sulphurous acid, and leaves a residue of sulphite and sulphate of barium. The aqueous solution presents a gummy mass on evaporation.

A further addition of alcohol to the mother-liquor of the sulphobutyrate of barium throws down a flocculent precipitate, which is

very soluble in water. It was purified by repeated and partial precipitation with alcohol. This substance, when dried at a temperature of 165° C., furnished upon analysis numbers agreeing with the expression

$$C_6\,(H_6\,Ba_2)\,S_4\,O_{12},$$

which is that of disulphopropiolate of barium.

Phenyl-Series.

Action of Sulphuric Acid upon Benzonitrile.

From the results obtained in the study of the methyl-, ethyl-, and propyl-series, we may fairly infer that all the homologues of other groups will exhibit a similar deportment.

It appeared, however, desirable to extend our researches to a class of bodies which are analogous (not homologous) to the preceding series.

Sulphuric acid appears to act with much less energy upon benzonitrile or cyanide of phenyl than upon the foregoing nitriles. No evolution of gas is observed until the mixture of the two substances is strongly heated, and then so much sulphurous acid is formed from charring of the cyanide that its presence is almost entirely masked.

The digestion was continued for two hours, after which the dark residue was treated in the usual manner for the soluble barium-salts. That first obtained consisted of sulphobenzoate of barium. For identification, both the neutral and acid compounds were prepared, the respective formulæ of which,

$$C_{14}\,(H_4\,Ba_2)\,S_2\,O_{10},$$

and

$$C_{14}\,(H_5\,Ba)\,S_2\,O_{10},$$

were corroborated by analysis.

An examination of the mother-liquor proved, as we had anticipated, the presence of a more soluble salt. By rapid evaporation it appears as an amorphous mass, but a drop allowed to dry spontaneously on the stage plate of the microscope exhibits it in the form of minute shuttle-shaped crystals.

The formula of this body, deduced from a sulphur- and barium-determination, is

$$C_{12}\,(H_4\,Ba_2)\,S_4\,O_{12},$$

which characterizes it as disulphobenzolate of barium.

The question naturally suggested itself, whether the same acid could not be prepared directly from the hydrocarbon benzol.

Mitscherlich has already described an acid (the sulphobenzolic) in which one equivalent of that hydrocarbon is associated with one equivalent of bibasic sulphuric acid. We have proved by experiment that sulphobenzolic acid takes up the elements of an additional equivalent when it is submitted to the prolonged action of Nordhausen sulphuric acid, and that, in fact, the union of these two bodies presents the most ready method of procuring disulphobenzolic acid in a state of purity.

The preceding researches establish in two different groups of bodies the existence of a series of bibasic acids containing four equivalents of sulphur, and which, irrespectively of any special view regarding their molecular arrangement, may be represented as formed by the association of the hydrocarbon (corresponding to marsh-gas) of the various groups with four equivalents of anhydrous sulphuric acid,—

Disulphometholic acid ...... $C_2\ H_4\,4SO_3$.
Disulphetholic acid......... $C_4\ H_6\,4SO_3$.
Disulphopropiolic acid...... $C_6\ H_8\,4SO_3$.
Disulphobenzolic acid ...... $C_{12}\,H_6\ 4SO_3$.

An acid of analogous composition exists in the naphthalin-series, disulphonaphtholic acid, $C_{20}\,H_8\,4SO_3$, which was discovered by Berzelius, and subsequently studied by Laurent. Many of these substances may be actually obtained directly from the hydrocarbons by the action of sulphuric acid.

On the other hand, chemists are well acquainted with the deportment of olefiant gas under the influence of anhydrous sulphuric acid. The crystalline compound discovered by Magnus, and described by him under the name of sulphate of carbyl, whatever its constitution may be, can be considered as a direct combination of olefiant gas, with four equivalents of anhydrous sulphuric acid,—

Sulphate of carbyl......... $C_4\,H_4\,4SO_3$.

It can scarcely be doubted that all the other hydrocarbons, $C_{n2}\,H_{n2}$, propylene, butylene, amylene, &c., will furnish homologous substances.

Sulphate of carbyl, when submitted to the action of water, assimi-

lates two equivalents, and is converted into a bibasic acid (ethionic), $C_4H_4 4SO_3 + 2HO = C_4H_6O_2 4SO_3$, which accordingly may be viewed as an association of alcohol with four equivalents of anhydrous sulphuric acid. Terms analogous to ethionic acid are sure to be found when the study of the homologues of sulphate of carbyl shall be taken up by chemists.

The production of disulpho-compounds of perfectly similar composition, from substances belonging to such different groups of bodies, as the hydrocarbons homologous and analogous to marsh-gas, ethylene and alcohol, suggested the possibility that the substances in question might be but individual examples illustrating a far more general mode of formation. It became, in fact, probable that all organic bodies capable of uniting with the elements of two equivalents of anhydrous sulphuric acid might, under favourable circumstances, be induced to assimilate two additional equivalents of anhydrous sulphuric acid, and thus furnish other terms belonging to the class of disulpho-compounds.

The only additional class of compounds to which we have as yet successfully extended our labours, is the group of organic bases.

Action of Sulphuric Acid upon Aniline.

The first product which sulphuric acid forms with aniline is simply the sulphate of the base.

A further addition of acid, assisted by heat, dissolves the sulphate, and after a sufficiently long digestion, the whole is converted into sulphanilic acid. If too high a temperature be employed, the acid becomes very dark in colour; and indeed there is a limit beyond which carbon is rapidly deposited, accompanied by the evolution of abundance of sulphuric acid.

The process of converting sulphanilic acid into the disulpho-compound is rather tedious. It may, however, be effected without fail, by treating for several hours the perfectly dry crystalline acid, mixed with strong Nordhausen acid, to the consistence of a paste. The heat should not exceed that at which sulphurous acid just commences to be evolved. We employed an air-bath heated from 160° to 170° C., and the digestion was continued until a portion removed by a glass rod showed no trace of crystallization when cooled or moistened with water.

Treatment with carbonate of barium yielded in this manner a very soluble salt, which furnished a brittle gum when evaporated. The new substance was precipitated from its solution by alcohol and dried at 200° C. A determination of the barium and the sulphur led to the expression

$$C_{12}\,(H_5\,Ba_2)\,N\,S_4\,O_{12},$$

which is the formula of disulphanilate of barium.

This salt is readily attacked by concentrated nitric acid with oxidation of the sulphur. It blackens when strongly heated on platinum foil, and yields sulphurous acid without inflaming, a deportment in which it differs from sulphanilate of barium, which burns with a bright flame. When heated in close vessels it forms a crystalline sublimate, which consists of sulphite of aniline.

Disulphanilic acid is prepared by decomposing the lead-salt with hydrosulphuric acid. It is very soluble in water, and crystallizes with difficulty. It may be precipitated from a strong aqueous solution by alcohol in the form of white grains. The precipitation is assisted by the addition of a little ether. It has a very rough and acid taste.

We have also prepared the silver-salt by saturating the acid with carbonate of silver. The most ready method of obtaining it in a solid state is by precipitation with alcohol and ether. The aqueous solution, by concentration, deposits a black powder which makes it very difficult to obtain the crystals colourless.

The formula of disulphanilate of silver is

$$C_{12}\,(H_5\,Ag_2)\,N\,S_4\,O_{12}.$$

The potassa-salt is crystalline. It forms small grains or minute needles, which are insoluble in alcohol.

The researches detailed in the preceding paragraphs may serve to characterize more fully a class of compounds of which only a few terms, isolated and scattered in widely different groups, had been previously observed. The only disulpho-acids hitherto known, are Berzelius's and Laurent's disulphonaphthalic acid, Magnus's ethionic (disulphethylic) acid, and lastly, dithiobenzic acid, recently discovered by M. Kilkenkamp. To these we now add five new acids, belonging to several of the most important series of compounds:—

Disulphometholic acid ...... $C_2 H_4 S_4 O_{12}$.
Disulphetholic acid .... $C_4 H_6 S_4 O_{12}$.
Disulphopropiolic acid ...... $C_6 H_8 S_4 O_{12}$.
Disulphobenzolic acid ...... $C_{12} H_6 S_4 O_{12}$.
Disulphanilic acid .... $C_{12} H_7 N S_4 O_{12}$.

Our experiments point out, moreover, the universal occurrence, and the general mode of formation of these substances. All organic molecules, particularly in the nascent state, appear to be capable of assimilating the elements of either two or four equivalents of anhydrous acid.

The formation of the two groups of acids which are thus produced presents a great analogy with the production of the nitro-substitutes generated under the influence of nitric acid. All these compounds are generated with the elimination of water. In the action of nitric and sulphuric acid upon benzol, for instance, we have,

$$\underbrace{C_{12} H_6}_{\text{Benzol.}} + HNO_6 = \underbrace{C_{12} H_5 NO_4}_{\text{Nitrobenzol.}} + 2HO$$

$$\underbrace{C_{12} H_6}_{\text{Benzol.}} + 2HNO_6 = \underbrace{C_{12} H_4 N_2 O_8}_{\text{Dinitrobenzol.}} + 4HO$$

$$\underbrace{C_{12} H_6}_{\text{Benzol.}} + H_2 S_2 O_8 = \underbrace{C_{12} H_6 S_2 O_6}_{\text{Sulphobenzolic acid.}} + 2HO$$

$$\underbrace{C_{12} H_6}_{\text{Benzol.}} + 2H_2 S_2 O_8 = \underbrace{C_{12} H_6 S_4 O_{12}}_{\text{Disulphobenzolic acid.}} + 4HO$$

The analogy of these reactions is obvious.

The action of nitric acid upon organic bodies is by no means limited to the production of nitro-compounds corresponding to nitrobenzol and dinitrobenzol; frequently additional substitutes are formed with elimination of six, eight, and in a few isolated cases, even of ten equivalents of water. It is possible that analogous sulpho-compounds may exist. Hitherto, however, no substances have been observed in which the assimilation of sulphuric acid has gone further than in the disulpho-acids.

VI. "On Quantitative Measurement in Statical Electricity, and on some new Phenomena of Electrical Force." By Sir WILLIAM SNOW HARRIS, F.R.S. Received June 12, 1856.

(Abstract.)

The author observes, that number, weight, and measure are the foundation of all exact science, and that, as expressed by an eminent and learned writer (M. Quetelet), no branch of human knowledge can be held as being out of its infancy which does not in some way or the other frame its theories or correct its practice by reference to those elements; he was hence led to seek and establish such rigorous and exact quantitative processes in common electricity as would measure the quantity of electricity in operation; its attractive or repulsive force under given conditions, and its dynamical or current force when traversing bodies under the form of electrical discharge. The instruments which he has invented for this purpose have been all honoured by a place in the 'Philosophical Transactions of the Royal Society.' They amount to five in number, viz. the Unit Measure, the Balance and Hydrostatic Electrometers, the Thermo-electrometer, and the Bifilar Balance. In referring to such of these instruments as are employed in the present research, the author briefly adverts to their general construction, including the latest and best form under which they have been placed.

In the measurement of quantity, he considers the unit measure as being the best and most accurate means of estimating quantity as yet arrived at; and he describes a series of crucial experiments, the object of which is to show that the unit explosions are rigorously exact. If an electrical jar exposing about 5 square feet of coated surface be insulated, and a second equal and similar jar be so placed as to charge from its outer coating, and if the first jar be charged from the conductor of the machine through the unit measure, it is found by a Lane's discharging electrometer attached to each jar, that an equal number of measures are given off from the outer coating of the insulated jar at all periods of the progress of the charge. Thus, whether the first jar be charged with 20, or 40, or 60 measures,

it still evolves from its outer coating the same number of measures for each unit of quantity as it did at first; and conversely, the second jar receives as easily the unit of quantity taken in terms of the unit explosions when charged with 20, 40, or 60 measures, as it did at first. The author concludes, that the charging of an electrical jar is by a rough analogy rather to be associated with the pouring of an inelastic fluid such as water into an open vessel in measured quantities, which is done up to the point of overflow as easily at last as at the first.

Having given experimental illustrations of the nature of the several instruments just adverted to, and shown their accuracy as instruments of research, the paper proceeds to consider the phenomena of what the author, after the learned Mr. Cavendish, denominates electrical charge. By the term electrical charge of a given conducting substance, the author understands the quantity of electricity which the body can sustain under a given degree of the electrometer. In pursuing this interesting question, he commences with an examination of the charges of hollow spheres or globes of different diameters. The method of experiment is to place the given sphere in communication with the electrometer, and find by a transfer of measured quantities of electricity the precise number of measures required to bring the index to a given degree of the arc. These measures are obtained by insulated balls or plates of given dimensions, brought into contact with the ball of an insulated charged jar carefully prepared and screened from the external air. The author shows how this method of measuring quantity by means of what he terms a quantity-jar may be perfected, so as to be relied on as a means of estimating small quantities of electricity.

The results of a series of experiments with spheres and plates of equal area led to the deduction, that the charges of these bodies are as the square roots of the surfaces multiplied into the circumferences, and that the charge of a sphere is to the charge of a circular plate of equal surface as $1 : \sqrt{2}$, and the charge of a great circle of a sphere is to the charge of the sphere as $1 : \sqrt{4}$, or $1 : 2$.

Taking a given surface of 100 square inches, and placing it under various forms, viz. a sphere, circular plate, square plate, rectangular plates of variable extension, a hollow open cylinder, a cube, &c., and subjecting these to the same process of experiment by which is

measured the quantity of electricity which each can sustain under a given degree of the electrometer or what the author calls intensity, he deduces the following:—

1. The charges of spheres and circular planes, as also of plane rectangular plates, are as the square roots of these surfaces multiplied into their circumferences or perimeters.

2. The charge of a cylinder is as the square root of its surface multiplied into the sum of its length and circumference.

3. The charge of a cube is as the square root of its surface multiplied into twice its side.

4. The charge of a sphere is to the charge of one of its great circles as 2 : 1.

5. The charge of a sphere is to the charge of a plane circle of equal surface as $1 : \sqrt{2}$, or as 1 : 1·41.

6. The charge of a cube is to the charge of a sphere of equal surface as 1 : 1·47 nearly.

7. The charge of a square plate is to the charge of a cube whose side equals the side of the square as 1 : 1·6 nearly.

8. The charge of a circular plate is to the charge of a square plate whose side equals the diameter of the circle as 1 : 1·28.

The author examines the charges of cylindrical rods or tubes of small diameter, and finds their capacity to be nearly as the length, the surface being constant; being quite in accordance with the result arrived at by Volta, who found that an insulated conductor composed of gilded rods could receive under the same intensity as much electricity as would produce a shock equal to a given extent of coated glass.

In referring to the beautiful experiments of Coulomb, the author conceives that the sharing of electricity between a circular plate and sphere of equal area, in proportion to the two surfaces of the plate to the one exterior surface of the sphere, is a different thing from the absolute charging of the plate on two surfaces, and adduces experiments to show that when a circular plate charges on both its surfaces, it takes up twice the quantity of electricity under the same intensity, which a plane circular plate in respect of a sphere of equal area does not; he conceives the sharing of electricity between a sphere and circular plate of equal area to be a pure result of the inductive susceptibility of the plate in consequence of the free exposure of its

entire surface; he further gives some new experiments on induction, with a view of proving that when one surface is opposed as it were to itself, as in the case of the interior surface of a sphere, the inductive susceptibility of one-half the surface is reduced to zero. The phenomena of what the author calls, after Cavendish, electrical charge, he refers to some peculiar arrangement or disposition of the electrical particles on the surfaces of the several conductors, by which they exhibit a greater or less degree of excitement, as observable by the electrometer.

The remaining portion of this paper is devoted to the laws and phenomena of electrical attractive force. The attractive force of a given surface under a given charge does not depend on the quantity of electricity, but on the number of attracting points called into operation by what is usually considered as the attracted body. Two circular discs of very light wood, of 5 inches diameter, being carefully prepared in a lathe, were divided into six concentric rings, including a central plate of about an inch in diameter. The attractive force on each pair of rings was determined by means of the electrical balance and carefully noted; the force was as the several opposed areas; and when the series was combined into one plate, the force was the sum of the forces of the respective rings; when the attractive forces of circular plates equal in area to the several rings respectively were examined, the force was the same as that exhibited by the two rings whose area was the same; hence it is inferred that whether the charge operates from the circumference or near the centre, the attractive force is the same. Two rings combined exhibit forces equal to the sum of the forces taken separately; and when the force is examined between the plates or the several rings and a plane circular area of large and continuous surface, the forces are no greater than that between two plates or rings of equal area. When the distances between the attracting surfaces or the quantities of the electrical accumulation varied, then the force was as the square of the accumulation directly, and as the square of the distances inversely.

The author extends these experiments to spheres of different diameters. He had shown in a former paper, that, taking the attractive force to be as the areas directly, and as the squares of the distances inversely, according to the expression $F \propto \frac{A}{D^2}$, two points

might be determined within the hemispheres in which all the force may be conceived to be collected, and to be the same as if proceeding from every point of the hemisphere. If Z=the distance of either point, $q\,q'$ taken within the hemisphere, r=radius, and a=distance between the near or what may be termed the touching points of the spheres, then we have $Z=\frac{(2ar+a^2)^{\frac{1}{2}}-a}{2}$; and if A=the surface, we have $F \propto \frac{A}{(a+2z)^2}$. When both hemispheres are equal, and distance $=a$ variable, then we have also $F \propto \frac{1}{a(a+2r)}$. The author in a former paper had applied these formulæ to the limited induction of a sphere of an inch radius; he now extends the inquiry to spheres varying from an inch to 5 inches or more in diameter, and finds the results conformable to the formulæ. He gives a table containing the results of a series of experiments with four spheres whose areas regularly increased, and the radii of which were from 1 to 2 inches in diameter. These were examined by the electrical balance. They were first placed with the points $q\,q'$, or centres of force as calculated for each at a constant distance of 1·1 of an inch, in which case the weights requisite to balance the force with a given number of measures of electricity were as the opposed areas, thus confirming the preceding results deduced with plane surfaces; when the distances were varied, the force varied as the squares of the distances between the centres of force, or according to the formula $F \propto \frac{1}{a(a+2r)}$.

With the view of further verifying these results, a set of plane circular plates in pairs, each pair equal in area to the areas of the respective hemispheres of the spheres, were submitted to experiment at the same constant distance 1·1, so as to cut the points $q\,q'$, or centres of force of the spheres; the attractive forces were found precisely the same as that of the opposed spheres to which the particular plates belonged.

The author has examined at various times and with very rigid attention, the several conditions under which electrical attractive force conforms to the law of force as deduced by Cavendish and Coulomb, and other eminent philosophers, and he finds this law true only for charged and neutral conductors of large inductive capacity; if either of the attracting surfaces have a narrow or limited susceptibility of

inductive charge, then this law of force no longer obtains. If, for example, the attracting plates be taken as mere planes of small thickness, and even although they be charged with opposite electricity, still in changing the distances between them we do not obtain at all distances a law of force in the inverse duplicate ratio of the distance such as we have found to obtain in other circumstances. The force will be commonly in an inverse simple ratio of the distance. If the neutral or suspended plane be taken very thin and insulated, then little or no attractive force is observable under any circumstances. If we continue to increase its thickness, then, as the author has shown in former papers*, attractive force begins to display itself, and will approach a law of change in the inverse duplicate ratio of the distance as we extend its dimensions. When we give it unlimited electrical extension by placing it in communication with the ground, then the force is as the square of the distance inversely; but it is not always so, until we effect this extension perfectly. When all these sources of disturbance are duly considered, it will not be difficult to reconcile the many conflicting results arrived at by several eminent philosophers in past times, in their endeavours to investigate the law of electrical force, and explain how, without any defect in their experimental processes, such conflicting results might arise. Volta, for example, found electrical force to vary in a simple inverse ratio of the distance. M. Simon, of Berlin, an eminent philosopher, and eulogized by Gilbert as being "remarkable for his dexterity and careful manipulation" in this branch of physics, failed to verify Coulomb's result, although he employed a new and very delicate apparatus, by which the repulsive force between two spheres was very accurately and beautifully measured. In these experiments he found the force to vary as the distance inversely†.

* Phil. Trans. for 1834.

† Poggend. Annal. for 1808, cap. 3, p. 277, and Ann. de Chim. vol. lxix. First Series.

June 19, 1856.

The LORD WROTTESLEY, President, in the Chair.

The following gentlemen were admitted into the Society:—

John Carrick Moore, Esq.
Edmund Potter, Esq.
Henry Hyde Salter, M.D.

The following gentlemen were recommended by the Council for election as Foreign Members:—

Wilhelm Karl Haidinger.
Antonio Secchi.

The following communications were read:—

I. "On Colour-Blindness." By WILLIAM POLE, Esq., F.R.A.S., F.G.S., Memb. Inst. C. E. Communicated by CHARLES MANBY, Esq., F.R.S. Received June 7, 1856.

(Abstract.)

The author's object in this paper is to state his own case of colour-blindness, which he believes to be one of the most decided on record; to compare it with others of the same kind; and to show that the general phenomena attending this defect of vision are more uniform and consistent, as well as more easy of explanation, than is generally supposed.

For general information on the subject, reference is made to a work lately published by Dr. Wilson of Edinburgh, entitled "Researches on Colour-Blindness," in which a great number of cases are fully described, and the optical and physiological theories of the defect carefully discussed.

After stating reasons which justify a colour-blind person undertaking the investigation and description of his own case, the author gives a preliminary statement of his views in regard to the general theory and nomenclature of colours, adopting the ordinary hypothesis that red, blue, and yellow are the three primaries; a theory which, though it has been lately called in question, receives, it is considered, new support from the phenomena of the defect of vision under consideration.

Dr. Wilson describes colour-blindness as of three kinds, namely—

1. Inability to discern any colour except black and white. This is very rare.
2. Inability to discriminate between the nicer distinctions of colour. This is so common as to be apparently rather the rule than the exception.
3. The third variety is the only one here treated of. Its outward manifestation is the inability to distinguish between many of the colours most marked to normal eyes, and its most complete form is what is called *dichromic* vision; being total blindness to one of the three primary colours.

The description of a case of colour-blindness may either be confined to a statement of what may be called the *symptoms* of the malady, i. e. the effects it produces on the individual's judgment of colours; or it may go further, and endeavour to describe the positive nature of the *sensations* experienced, the causes, so to speak, of the outward symptoms observed. The first is the plan usually adopted, but the author combines both in the account of his own case.

As regards the outward symptoms, he finds them very similar to those of other cases; and for the purpose of showing this similarity, he collects in an appendix the principal features of nearly forty published cases, and points out that as a general rule he can corroborate the whole from his own impressions, the points where they appear to differ being very few and exceptional.

An abstract is then given of the symptoms exhibited, as collected from these cases. They are as follows:—

Blue and yellow are perfectly distinguished, and are the only colours seen in the spectrum.

Almost all colour-blind persons think they see red, but it is frequently confounded with green (the most common mistake),

black, orange, yellow, brown, blue and violet. Crimson and pink appear to have no relation to scarlet.

Green is a most perplexing colour; it is not only confounded with red, but with black, white, or grey, orange, yellow, blue, violet and brown.

Violet is confounded with blue or grey; and orange with yellow.

More difficulty is manifested with light or dark tones of compound colours than with full ones.

In explaining more accurately the real nature of the author's vision of colours, he employs as standard examples of reference the "Cercles Chromatiques," and "Gammes Chromatiques" of M. Chevreul (copies of which accompany the paper), the former giving various gradations of *hue*, the latter, gradations of *tone*. He states that his vision is perfectly *dichromic*, and shows the applicability to it of the definition of this kind of vision given by Sir John Herschel, which he believes has never hitherto been followed out so completely as is necessary to explain the phenomena observed.

Blue and *yellow* he sees perfectly well, and has no reason to doubt that his sensations of these two colours are the same as those of the normal-eyed. The third primary, *red*, is the one in regard to which his vision is defective, but the study of the sensations produced by this colour has been involved in some difficulty. Carmine, the artificial representative of what is usually considered pure red, presents to the author's eye a very positive sensation, which he long supposed to be a distinct colour; but on examining it more closely, he found it to be merely a dark shade of yellow, as he could match carmine red perfectly with a mixture of yellow and black. There is, however, a variety of red, namely *crimson*, which is perfectly invisible, as a colour, to his eyes, giving only a sensation of darkness; and the whole of the hues of red and orange between this and yellow present only different *shades* of the latter colour; the red element appearing to act, not as a colouring agent, but simply as a darkening power. The author has endeavoured to find the place of this, to him, neutral or invisible hue of red on the spectrum, and believes that if it exists there at all, it must be situated at one or both of the extreme ends, a position which would appear to distinguish it as possessing some peculiar property, and he offers a conjecture that this, and not carmine red, may perhaps be the true primary colour.

The hues of *violet*, lying between blue and crimson, appear, on a similar principle, only shades of blue, the red darkening the blue in the same manner as the yellow.

In passing on to the *green* division of the colour circle, lying between the blue and yellow, the author calls attention to the apparent anomaly, that though colour-blind persons see blue and yellow perfectly well, their combination, green, should be so great a stumbling-block. This fact appears to have perplexed everybody who has treated on the subject; the author imagined he was the first to discover the explanation, but he found he had been anticipated by Sir John Herschel, who says in his letter to Dalton, "the equilibrium of blue and yellow *produces your white*," i.e. the white of the colour-blind is not white at all, but *green*. And this is consistent with theory; for if normal white is a combination of three elements, the invisibility of one of these elements to the colour-blind should naturally have the effect of changing the appearance of their compound. Since, therefore, green is only a *colour* to the normal-eyed as it is contrasted with white light, it becomes no colour at all to the colour-blind. The author proves this by showing that a certain hue of green exactly matches, to his eyes, a neutral grey; that all greens on the yellow side of this appear only shades of yellow, all on the blue side, only shades of blue.

Thus the dichromic explanation of the author's vision is complete. He has only two sensations of colour, properly so called, namely blue and yellow, all other hues in nature being reduced to *shades* of these. The colour of light, or the hue resulting from their combination, may be called green, white, or grey, at pleasure. It is shown that this explanation of colour-blind vision will fully account for the whole of the various symptoms above enumerated. Red and green, for example, are both seen only as shades of yellow, and if the yellow is of the same intensity in each, they will appear alike, and of course be confounded with each other.

The author then proceeds to consider how far his own case may be regarded as a type of the defect in general. The varied and incongruous nature of the symptoms has given rise to a belief that there are many varieties of colour-blindness, or at least many different degrees of severity; but after carefully examining the published accounts, he has arrived at the conviction that the true di-

chromic affection is much more common than is generally supposed. He points out reasons why the descriptions given by the colour-blind of their sensations may often be imperfectly expressed and easily misunderstood, alludes to the difficulty of explaining the symptoms by any other hypothesis, or even of classifying them in any consistent way; and considers the fact exemplified in his own case, that dichromic vision will explain all the phenomena, as strongly corroborative of the uniformity insisted on.

From the results of his investigations, the author draws a few inferences in regard to the theory of the primary colours, although admitting his incompetence to deal fully with this part of the subject. He considers that, from the extreme simplicity of the phenomena of colours as seen by the colour-blind, their experience may serve as a stepping-stone to the more complex problems of normal vision. Their light is divisible into two colours, blue and yellow; and since these must be undoubtedly primaries to the colour-blind, it is reasonable to infer they should also be primaries in the normal system. The dichromic eye further becomes of use as an analyser of colours, and can detect the presence of blue or yellow in compounds whose elements may be inseparable to normal eyes. Thus it finds that in orange there is much yellow, and in violet much blue, and therefore these cannot be simple colours. Red, producing no impression on the colour-blind eye, may be assumed to be a simple colour, and may therefore be put down as the third primary, so that the phenomena of colour-blindness would appear to confirm the ordinary theory, or at least are more consistent with it than with any other. The fact of carmine-red presenting to the colour-blind a decided sensation of yellow, affords a confirmation of Sir David Brewster's theory of the triple spectrum, according to which this result ought to be expected.

The principal symptom of colour-blindness being the mistaking of red for green, and *vice versâ*, it has been thought that the use of these colours for railway and ship signals becomes dangerous where colour-blind persons may have to observe them. The author points out that this danger may be obviated by very simple means. Red and green are not confounded with each other generally, but only such hues of them as lie in both cases on the yellow side of the neutral; and therefore if the green be made a *blue* green at the same

time that the red is a *yellow* red, they become quite as distinct to the colour-blind as to the normal-eyed.

The colouring of geological maps is very perplexing to the colour-blind, and it is recommended that engraved marks, to distinguish the different strata, should always be added to the colours.

In conclusion, the author gives hints which he considers useful for the examination of colour-blind persons, and states the importance of collecting further evidence on the subject, of an accurate and definite nature.

II. "Researches on the Velocities of Currents of Air in Vertical Tubes, due to the presence of Aqueous Vapour in the Atmosphere." By W. D. Chowne, M.D. Communicated by John Bishop, Esq., F.R.S. Received May 22, 1856.

(Abstract.)

This was a paper supplementary to one presented June 14th, 1855, an abstract of which was published in the 'Proceedings of the Royal Society' for June 21st, 1855. The author having ascertained that an upward current of air becomes established in a vertical tube placed in as quiescent an atmosphere as can be obtained, and having demonstrated its existence by means of anemometric discs placed in tubes as described in that paper, proceeded to ascertain the velocity of the currents by which the discs were moved.

In order to estimate the velocity of the currents, one of the anemometric discs was placed within a short zinc tube three inches in diameter, the lower end of which was accurately fitted into an aspirator capable of containing thirty-six gallons of water. By drawing off in a given time a quantity of water equal in bulk to the cubic contents of one of the tubes described in the former paper, the velocity of a current required to produce a given number of rotations of the disc was determined.

The experiments were varied by altering the height of water in the aspirator, and thereby changing the velocity, while the exit-orifice remained unaltered.

By ascertaining the number of rotations of the anemometric disc,

caused by currents of air of different velocities thus produced, he was enabled to arrive at a measure of the velocities in tubes placed in a still atmosphere, as described in his former paper.

The author in that paper pointed out a correspondence between the variations of force in the upward currents of atmospheric air in the tubes and variations in the humidity of the atmosphere, and expressed his belief that the variations were attributable in great measure to the varying hygrometric conditions of the atmosphere.

In further proof of this position, he has appended two tables, showing that both natural and artificial increase of atmospheric humidity are accompanied by increase in the velocity of the rotations, and that in each case increase of humidity is attended by increase of velocity, independent of temperature.

III. "On the Thermal Effects of Fluids in Motion." By J. P. Joule, Esq., F.R.S., and Professor W. Thomson, F.R.S. Received May 23, 1856.

On the Temperature of Solids exposed to Currents of Air.

In examining the thermal effects experienced by air rushing through narrow passages, we have found, in various parts of the stream, very decided indications of a lowering of temperature (see Phil. Trans. June 1853), but never nearly so great as theoretical considerations at first led us to expect, in air forced by its own pressure into so rapid motion as it was in our experiments. The theoretical investigation is simply as follows:—Let P and V denote the pressure and the volume of a pound of the air moving very slowly up a wide pipe towards the narrow passage. Let p and v denote the pressure and the volume per pound in any part of the narrow passage, where the velocity is q. Let also $e-\mathrm{E}$ denote the difference of intrinsic energies of the air per pound in the two situations. Then the equation of mechanical effect is

$$\frac{q^2}{2g}=(\mathrm{PV}-pv)+(\mathrm{E}-e),$$

since the first member is the mechanical value of the motion, per

pound of air; the first bracketed term of the second member is the excess of work done in pushing it forward, above the work spent by it in pushing forward the fluid immediately in advance of it in the narrow passage; and the second bracketed term is the amount of intrinsic energy given up by the fluid in passing from one situation to the other.

Now, to the degree of accuracy to which air follows Boyle's and Gay-Lussac's laws, we have

$$pv = \frac{t}{T} PV,$$

if t and T denote the temperatures of the air in the two positions reckoned from the absolute zero of the air-thermometer. Also, to about the same degree of accuracy, our experiments on the temperature of air escaping from a state of high pressure through a porous plug, establish Mayer's hypothesis as the thermo-dynamic law of expansion; and to this degree of accuracy we may assume the intrinsic energy of a mass of air to be independent of its density when its temperature remains unaltered. Lastly, Carnot's principle, as modified in the dynamical theory, shows that a fluid which fulfils those three laws must have its capacity for heat in constant volume constant for all temperatures and pressures,—a result confirmed by Regnault's direct experiments to a corresponding degree of accuracy. Hence the variation of intrinsic energy in a mass of air is, according to those laws, simply the difference of temperatures multiplied by a constant, irrespectively of any expansion or condensation that may have been experienced. Hence, if N denote the capacity for heat of a pound of air in constant volume, and J the mechanical value of the thermal unit, we have

$$E - e = JN(T - t).$$

Thus the preceding equation of mechanical effect becomes

$$\frac{q^2}{2g} = PV\left(1 - \frac{t}{T}\right) + JN(T - t).$$

Now (see "Notes on the Air-Engine," Phil. Trans. March 1852, p. 81, or "Thermal Effects of Fluids in Motion," Part 2, Phil. Trans. June 1854, p. 361) we have

$$JN = \frac{1}{k-1} \frac{H}{t_0}\Big| = \frac{1}{k-1} \frac{PV}{T},$$

where k denotes the ratio of the specific heat of air under constant pressure to the specific heat of air in constant volume; H, the product of the pressure into the volume of a pound, or the "height of the homogeneous atmosphere" for air at the freezing-point (26,215 feet, according to Regnault's observations on the density of air), and t_0 the absolute temperature of freezing (about 274° Cent.). Hence we have

$$\frac{q^2}{2g} = \mathrm{PV}\left(1 + \frac{1}{k-1}\right)\left(1 - \frac{t}{\mathrm{T}}\right) = \frac{k\,\mathrm{PV}}{k-1}\left(1 - \frac{t}{\mathrm{T}}\right).$$

Now the velocity of sound in air at any temperature is equal to the product of $\sqrt{k}$ into the velocity a body would acquire in falling under the action of a constant force of gravity through half the height of the homogeneous atmosphere; and therefore if we denote by a the velocity of sound in air at the temperature T, we have

$$a^2 = kg\mathrm{PV}.$$

Hence we derive from the preceding equation,

$$\frac{\mathrm{T}-t}{\mathrm{T}} = \frac{k-1}{2}\left(\frac{q}{a}\right)^2,$$

which expresses the lowering of temperature, in any part of the narrow channel, in terms of the ratio of the actual velocity of the air in that place to the velocity of sound in air at the temperature of the stream where it moves slowly up towards the rapids. It is to be observed, that the only hypothesis which has been made is, that in all the states of temperature and pressure through which it passes the air fulfils the three gaseous laws mentioned above; and that whatever frictional resistance, or irregular action from irregularities in the channel, the air may have experienced before coming to the part considered, provided only it has not been allowed either to give out heat or to take in heat from the matter round it, nor to lose any mechanical energy in sound, or in other motions not among its own particles, the preceding formulæ will give the lowering of temperature it experiences in acquiring the velocity q. It is to be observed that this is not the velocity the air would have in issuing in the same quantity at the density which it has in the slow stream approaching the narrow passage. Were no fluid friction operative in the circumstances, the density and pressure would be the same in

the slow stream flowing away from, and in the slow stream approaching towards the narrow passage; and each would be got by considering the lowering of temperature from T to t as simply due to expansion, so that we should have

$$\frac{t}{T} = \left(\frac{V}{v}\right)^{k-1}$$

by Poisson's formula. Hence if Q denote what we may call the "reduced velocity" in any part of the narrow channel, as distinguished from q, the actual or true velocity in the same locality, we have

$$Q = \frac{V}{v} q = \left(\frac{t}{T}\right)^{\frac{1}{k-1}} q,$$

and the rate of flow of the air will be, in pounds per second, wQA, if w denote the weight of the unit of volume, under pressure P, and A the area of the section in the part of the channel considered. The preceding equation, expressed in terms of the "reduced velocity," then becomes

$$1 - \frac{t}{T} = \frac{k-1}{2} \left(\frac{T}{t}\right)^{\frac{2}{k-1}} \left(\frac{Q}{a}\right)^2,$$

and therefore we have

$$\frac{Q}{a} = \sqrt{\left\{\frac{2}{k-1} \left(\frac{t}{T}\right)^{\frac{2}{k-1}} \left(1 - \frac{t}{T}\right)\right\}}.$$

The second member, which vanishes when $t = 0$, and when $t = T$, attains a maximum when

$$t = {\cdot}83\,T,$$

the maximum value being

$$\frac{Q}{a} = {\cdot}578.$$

Hence, if there were no fluid friction, the "reduced velocity" could never, in any part of a narrow channel, exceed ·578 of the velocity of sound, in air of the temperature which the air has in the wide parts of the channel, where it is moving slowly. If this temperature be 13° Cent. above the freezing-point, or 287° absolute temperature (being 55° Fahr., an ordinary atmospheric condition), the velocity of sound would be 1115 feet per second, and the maximum reduced velocity of the stream would be 644 feet per second. The cooling

effect that air must, in such circumstances, experience in acquiring such a velocity would be from 287° to 268° absolute temperature, or 19° Cent.

The effects of fluid friction in different parts of the stream would require to be known in order to estimate the reduced velocity in any narrow part, according to either the density on the high-pressure side or the density on the low-pressure side. We have not as yet made any sufficient investigation to allow us to give even a conjectural estimate of what these effects may be in any case. But it appears improbable that the "reduced velocity," according to the density on the high-pressure side, could ever with friction exceed the greatest amount it could possibly have without friction. It therefore seems improbable that the "reduced velocity" in terms of the density on the high-pressure side can ever, in the narrowest part of the channel, exceed 644 feet per second, if the temperature of the high-pressure air moving slowly be about the atmospheric temperature of 13° Cent. used in the preceding estimate.

Experiments in which we have forced air through apertures of $\frac{29}{1000}$, $\frac{53}{1000}$, and $\frac{84}{1000}$ths of an inch in diameter drilled in thin plates of copper, have given us a maximum velocity reduced to the density of the high-pressure side equal to 550 feet per second. But there can be little doubt that the stream of air, after issuing from an orifice in a thin plate, contracts as that of water does under similar circumstances. If the velocity were calculated from the area of this contracted part of the stream, it is highly probable that the maximum velocity reduced to the density on the high-pressure side would be found as near 644 feet as the degree of accuracy of the experiments warrants us to expect.

As an example of the results we have obtained on examining the temperature of the rushing stream by a thermo-electric junction placed $\frac{1}{8}$th of an inch above the orifice, we cite an experiment, in which the total pressure of the air in the receiver being 98 inches of mercury, we found the velocity in the orifice equal to 535 and 1780 feet respectively as reduced to the density on the high-pressure and that on the atmospheric side. The actual velocity in the small aperture must have been greater than either of these, perhaps not much greater than 1780, the velocity reduced to atmospheric density. If it had been only this, the cooling effect would have been

exactly $T\frac{k-1}{2}\left(\frac{1780}{1115}\right)^2$, that is, a lowering of temperature amounting to 150° Cent. But the amount of cooling effect observed in the experiment was only 13° Cent.; nor have we ever succeeded in observing (whether with thermometers held in various positions in the stream, or with a thermo-electric arrangement constituted by a narrow tube through which the air flows, or by a straight wire of two different metals in the axis of the stream, with the junction in the place of most rapid motion, and in other positions on each side of it,) a greater cooling effect than 20° Cent; we therefore infer *that a body round which air is flowing rapidly acquires a higher temperature than the average temperature of the air close to it all round.* The explanation of this conclusion probably is, that the surface of contact between the air and the solid is the locality of the most intense frictional generation of heat that takes place, and that consequently a stratum of air round the body has a higher average temperature than the air further off; but whatever the explanation may be, it appears certainly demonstrated that the air does not give its own temperature even to a tube through which it flows, or to a wire or thermometer-bulb completely surrounded by it.

Having been convinced of this conclusion by experiments on rapid motion of air through small passages, we inferred of course that the same phenomenon must take place universally whenever air flows against a solid or a solid is carried through air. If a velocity of 1780 feet per second in the foregoing experiment gave 137° Cent. difference of temperature between the air and the solid, how probable is it that meteors moving at from six to thirty miles per second even through a rarefied atmosphere, really acquire, in accordance with the same law, all the heat which they manifest! On the other hand, it seemed worth while to look for the same kind of effect on a much smaller scale in bodies moving at moderate velocities through the ordinary atmosphere. Accordingly, although it has been a practice in general undoubtingly followed, to whirl a thermometer through the air for the purpose of finding the atmospheric temperature, we have tried and found, with thermometers of different sizes and variously shaped bulbs, whirled through the air at the end of a string, with velocities of from 80 to 120 feet per second, temperatures always higher than when the same thermometers are whirled in

exactly the same circumstances at smaller velocities. By alternately whirling the same thermometers for half a minute or so fast, and then for a similar time slow, we have found differences of temperature sometimes little if at all short of a Fahrenheit degree. By whirling a thermo-electric junction alternately fast and slow, the same phenomenon is most satisfactorily and strikingly exhibited by a galvanometer. This last experiment we have performed at night, under a cloudy sky, with the galvanometer within doors, and the testing thermo-electric apparatus whirled in the middle of a field; and thus, with as little as can be conceived of disturbing circumstances, we confirmed the result we had previously found by whirling thermometers.

Velocity of Air escaping through narrow Apertures.*

In the foregoing part of this communication, referring to the circumstances of certain experiments, we have stated our opinion that the velocity of atmospheric air impelled through narrow orifices was, in the narrowest part of the stream, greater than the reduced velocity corresponding to the atmospheric pressure; in other words, that the density of the air, kept at a constant temperature, was, in the narrowest part, less than the atmospheric density. In order to avoid misconception, we now add, that this holds true only when the difference of pressures on the two sides is small, and friction plays but a small part in bringing down the velocity of the exit stream. If there is a great difference between the pressures on the two sides, the reduced velocity will, on the contrary, be *less* than that corresponding with the atmospheric pressure; and even if the pressure in the most rapid part falls short of the atmospheric pressure, the density may, on account of the cooling experienced, exceed the atmospheric density.

We stated that, at 57° Fahr., the greatest velocity of air passing through a small orifice is 550 feet per second, if reduced to the density on the high-pressure side. The experiments from which we obtained this result enable us also to say that this maximum occurs, with the above temperature and a barometric pressure of 30·14 inches, when the pressure of the air is equal to about 50 inches of mercury above the atmospheric pressure. At a higher or lower pressure, a smaller volume of the compressed air escapes in a given time.

* Received June 19, 1856.

Surface Condenser.—A three-horse power high-pressure steam-engine was procured for our experiments. Wishing to give it equal power with a lower pressure, we caused the steam from the eduction port to pass downwards through a perpendicular iron gas-pipe, ten feet long and an inch and a half in diameter, placed within a larger pipe through which water was made to ascend. The lower end of the gas-pipe was connected with the feed-pump of the boiler, a small orifice being contrived in the pump cover in order to allow the escape of air before it could pass, along with the condensed water, into the boiler. This simple arrangement constituted a "surface condenser" of a very efficient kind, giving a vacuum of 23 inches, although considerable leakage of air took place, and the apparatus generally was not so perfect as subsequent experience would have enabled us to make it. Besides the ordinary well-known advantages of the "surface condenser," such as the prevention of incrustation of the boiler, there is one which may be especially remarked as appertaining to the system we have adopted, of causing the current of steam to move in an opposite direction to that of the water employed to condense it. The refrigerating water may thus be made to pass out of the condenser at a high temperature, while the vacuum is that due to a low temperature; and hence the quantity of water used for the purpose of condensation may be materially reduced. We find that our system does not require an amount of surface so great as to involve a cumbrousness or cost which would prevent its general adoption, and have no doubt that it will shortly supersede that at the present time almost universally used.

IV. "On the Stability of Loose Earth." By W. J. MACQUORN RANKINE, Esq., C.E., F.R.SS. L & E., Regius Professor of Civil Engineering and Mechanics in the University of Glasgow.

(Abstract.)

The object of this paper is to deduce the mathematical theory of that kind of stability which depends on the mutual friction of the parts of a granular mass devoid of tenacity, from the known laws of friction, unaided by any hypothesis.

The fundamental principle of the internal stability of such a mass has already been published in the 'Proceedings of the Royal Society' for the 6th of March, 1856, viz. that the ratio of the difference to the sum of the greatest and least pressures at each point of the mass must not exceed the sine of the angle of repose.

The principles of the general theory of the internal equilibrium of a solid mass are expressed in a form suited to the special subject of the paper. For the purpose of determining the conditions of equilibrium under its own weight, of a solid mass whose upper surface is that generated by the motion of a horizontal straight line along a line of any figure described on a vertical plane at right angles to the generating line, the mass is supposed to be divided into *layers of equal horizontal thrust* by a series of surfaces, which layers are subdivided into elementary horizontal prisms by vertical planes normal to the vertical plane first mentioned. For independent variables there are taken the horizontal coordinate in this plane, and the total horizontal thrust from the upper surface down to a given surface of equal thrust. The condition of equilibrium of any one of the before-mentioned elementary prisms being expressed by a differential equation in terms of those variables, the integration of that equation gives the vertical coordinate of any surface of equal thrust in terms of the total thrust down to that surface and of the horizontal coordinate. The integral obtained belongs to a class first investigated by Fourier.

An approximation to the forms of the surfaces of equal thrust is obtained by a simple graphic process, first employed by Prof. William Thomson in connexion with the theory of electricity.

It is shown incidentally how the same integral may be applied to determine the intrados from the extrados of any arched rib, loaded only with its own weight.

The pressure on a surface of equal thrust is vertical; the pressure on a vertical plane at a given point is parallel to the surface of equal thrust traversing that point. When the upper surface of the mass of earth is one plane, horizontal or inclined, the surfaces of equal thrust are planes parallel to it. When the upper surface presents elevations and depressions, the surfaces of equal thrust have corresponding elevations and depressions, gradually vanishing as the depth increases.

The principles of the paper are applied to the determination of the pressure of earth against walls, and the power of earth to sustain buildings. The weight of the building which a horizontal bed of earth will sustain, exceeds the weight of the earth displaced by the foundation, in a ratio which is a function of the angle of repose.

V. "On the Geometrical Isomorphism of Crystals." By HENRY JAMES BROOKE, Esq., F.R.S., Hon. M.C.P.S. &c. Received June 11, 1856.

(Abstract.)

The author commences by remarking that all the crystals at present known have been divided into the six following groups or systems:—the cubic, pyramidal, rhombohedral, prismatic, oblique, anorthic.

He then states that he has constructed tables which accompany this paper of the minerals comprised in each of these systems, except the cubic, in a manner new, as he believes, to crystallography; and that the unexpected facts exhibited by the tables present that science under a new aspect.

The author explains briefly the language and notation he employs in discussing the results of the new tables.

It appears that the crystals in each system, except the cubic, are distinguished from each other by what are termed their elementary angles, that is by angles between particular faces of what may be termed elementary forms.

It is next observed that there is not in crystals any natural character which indicates an elementary or primary form, and it is shown that cleavage which Hauy regarded as such an indication, is only a physical character depending upon the degree of force with which the crystalline particles cohere at the surfaces of particular faces.

The question of high indices is also considered with reference to their influence on the choice of an elementary or primary form, and a general explanation is given of the nature of such indices.

The author then states that the most important of the facts presented by these tables, are the horizontal ranges of nearly equal

angles, as shown in each system, and the general disagreement in the symbols hitherto assigned to the faces which make with some other face those nearly agreeing angles.

With regard to these facts he observes that no difference of opinion can arise, unless the sources from which they have been derived are incorrect; but that differences of opinion may be entertained relative to the interpretation of them.

The interpretation to which the author inclines is, that the near agreement in angle between two corresponding faces is not simply accidental, but that it is the effect of some natural relation not hitherto noticed, among all the crystals in each respective system; and hence, that where the angles between particular faces nearly agree, there ought to be a corresponding agreement in the forms of their symbols.

With this view of the subject in his mind, it occurred to the author that there might be a similar agreement among the whole of the elementary angles in each system, and an examination of the crystals in the pyramidal and rhombohedral systems to ascertain how far this conjecture might be well-founded, has shown that a geometrical isomorphism does exist throughout each of these systems, and that similar relations may therefore be imagined to exist in the other systems.

The author has also suggested that the oblique and anorthic systems are only hemihedral and tetartohedral varieties of prismatic crystals.

VI. "On some Compounds of Ethylene." By H. L. Buff. Communicated by A. W. Hofmann, Ph.D., F.R.S. Received June 10, 1856.

Among the hydrocarbons which are capable of replacing hydrogen, the radicals of the general formula $C_n H_{(n+1)}$, *i. e.* the homologues of ethyl, are best examined. There is another class of hydrocarbons which may be represented by the general formula $C_n H_{(n-1)}$. The only well-known term of this series is the radical allyl, $C_6 H_5$, to which the attention of chemists has been especially called of late by

the researches of Messrs. Hofmann and Cahours on allylic alcohol. These researches have established the most perfect parallelism between the two classes of radicals and their derivatives. Both the radicals $C_n H_{(n+1)}$ and $C_n H^{(n-1)}$ are *monatomic*, i.e. molecules capable of replacing 1 equiv. of hydrogen.

These two classes stand in the closest relation to each other, and it is by no means improbable that one class may pass over into the other, for instance, that the radical propyl $C_6 H_7$, or a propyl-compound, may be converted into allyl or an allyl-compound.

There exist a third series of hydrocarbons, which, again, both by composition and origin, are closely allied to the former two. They are represented by the general formula $C_n H_n$; and methylene, $C_2 H_2$, ethylene, $C_4 H_4$, and propylene, $C_6 H_6$, are well-known terms belonging to this series. These hydrocarbons are also radicals; they differ, however, in their nature essentially from those of the former groups, inasmuch as they are *biatomic* molecules, *i. e.* molecules capable of replacing 2 equivs. of hydrogen.

There exist parallel with these three series of radicals which form *alcohols*, three other groups of radicals, which *in acids* play exactly the same part that in the *alcohols* is assigned to the hydrocarbons. These acid-forming radicals contain, in addition to carbon and hydrogen, oxygen and other elements belonging to the oxygen group. They are closely connected with the radicals of the alcohols, and this close connexion is particularly well established between the first series of alcohol-forming radicals and the corresponding series of acid-forming radicals.

Methyl, $C_2 H_3 - 2H + 2O =$ Formyl, $C_2 HO_2$
Ethyl, $C_4 H_5 - 2H + 2O =$ Acetyl, $C_4 H_3 O_2$
Propyl, $C_6 H_7 - 2H + 2O =$ Propionyl, $C_6 H_5 O_2$.

Formic, acetic and propionic acids are formed by the imperfect oxidation of methyl-, ethyl- and propyl-alcohol, and we may consider them to be simple substitution-products of these alcohols.

By means of the electric current we are able to produce ethyl, methyl and hydrogen from propionic, acetic and formic acids, and these acids we may reproduce again by the action of hydrate of potassa on the cyanogen compounds of hydrogen, methyl and ethyl.

Both series of radicals are chained together by these reactions, and we may view acetyl and propionyl as formyl, the hydrogen of which is replaced by methyl and ethyl.

Formyl	$=C_2\,(H)\,O_2$
Acetyl	$=C_2\,(C_2\,H_3)\,O_2$
Propionyl........	$=C_2\,(C_4\,H_5)\,O_2$.

There is no doubt that the same relation exists between the hydrocarbons of the other series of radicals and the radicals of the corresponding acids, between allyl, $C_6\,H_5$, and the radical of acrylic acid, acryl $C_6\,H_5\,O_2$, and between methylene, $C_2\,H_2$, ethylene, $C_4\,H_4$, propylene, &c., and the radicals of the bibasic acids, which are homologues of succinic acid, $C_8\,H_6\,O_8$.

The biatomic radicals are in general far less studied than the monatomic radicals; still they occur in many compounds, and are met with in different departments of chemistry.

In addition to the terms already mentioned, we find them in the phenyl, benzyl, naphthyl and other series.

In the hope of adding some facts to the history of the polyatomic radicals, I have made some experiments with chloride of ethylene, $C_4\,H_4\,Cl_2$.

This compound, as well as the bromide of ethylene, refused to act in many instances; in others it underwent the same change which is induced by the action on it of a solution of potassa in alcohol, splitting into the compound $C_4\,H_3\,Cl$ and hydrochloric acid.

On boiling chloride or bromide of ethylene with an alcoholic solution of sulphocyanide of potassium, a very definite reaction takes place. The change being completed, the alcohol is separated by distillation, and the residue treated with a small quantity of cold water in order to remove chloride or bromide of potassium, which is produced, and the excess of sulphocyanide of potassium. The more or less coloured residue is then dissolved in boiling alcohol, and the solution, after digestion for some time with animal charcoal and a few drops of hydrochloric acid, filtered whilst hot. This solution deposits on cooling fine white, very brilliant and large rhombic plates of a hard and brittle substance*.

* Whilst M. Buff was engaged with these researches, M. Sonnenschein has communicated some experiments made in the same direction, which have likewise

The analysis of this substance leads to the formula

$$C_4\,H_4\,Cy_2\,S_4,$$

and its formation may be represented by the equation

$$C_4\,H_4\,Cl_2+2K\,Cy\,S_2=2K\,Cl+C_4\,H_4\,Cy_2\,S_4.$$

Sulphocyanide of ethylene fuses at 90° C. and solidifies at 83°. It is but slightly soluble in cold water, more so in boiling water, from which it crystallizes in groups of needles. It is decomposed at a higher temperature, and evolves a highly pungent vapour, the odour of which very much resembles that of burnt onions. On boiling a solution of sulphocyanide of ethylene in water, a very acrid odour is observed, which produces lacrymation and violent sneezing. Sulphocyanide of ethylene has a sharp taste, causing a burning sensation in the throat.

Solution of ammonia decomposes sulphocyanide of ethylene even at the common temperature. A flocculent substance separates, and the solution contains several compounds which I have been unable to separate.

At the temperature of boiling water sulphocyanide of ethylene mixes with aniline in every proportion ; no reaction, however, is perceptible. But on boiling the mixture decomposition sets in, and a volatile substance is evolved which restores the colour of reddened litmus paper.

When boiled with solution of hydrate of baryta and oxide of lead or mercury, sulphocyanide of ethylene loses its sulphur; the substance left behind possesses very little power of crystallizing. In the case of oxide of mercury, besides sulphide of mercury and carbonate of barium, a difficultly soluble body containing mercury is formed.

At the temperature of boiling water, sulphocyanide of ethylene dissolves readily in very dilute nitric acid; on cooling of the solution the substance is deposited unchanged. On treating it with stronger nitric acid a decomposition takes place, and a crystalline acid is formed. This acid is best produced by heating sulphocyanide of ethylene on the water-bath with dilute nitric acid as long as red

led to the discovery of this substance. Sonnenschein's results, which are published in the Journ. für Prakt. Chem. June 1855, came to our knowledge only after a summary of the results had been sent to the editor of the Annalen der Chem. und Pharm.—A. W. H.

fumes of nitrous acid are evolved. The residue is a strongly acid syrup, which becomes finally crystalline. It is repeatedly dissolved in water and evaporated on the water-bath in order to expel the nitric acid. Thus purified, the new acid is dissolved in boiling water, neutralized with pure carbonate of barium and separated from sulphate of barium. On cooling, the barium-salt of the new acid crystallizes. It is soluble in boiling water, less so in cold water, and almost insoluble in alcohol, by means of which it may be precipitated from its solution in water.

On examining the appearance and deportment of this salt, Dr. Hofmann, in whose laboratory I performed these experiments, at once recognized the identity of this compound with the barium-salt of disulphetholic acid which he and Mr. Buckton have lately discovered.

This view was fully confirmed by the analysis which I made.

The composition of the barium-salt, dried at 160°, is represented by the formula

$$C_4\,(H_4\,Ba_2)\,S_4\,O_{12}.$$

The potassium salt of this acid is readily soluble in water; it crystallizes easily, and is likewise precipitated by alcohol from its solution in water. Dried at 100° C., it contains

$$C_4\,(H_4\,K_2)\,S_4\,O_{12}.$$

At 160° it suffers no decomposition; when exposed to a higher temperature, however, it blackens and intumesces, empyreumatic substances being evolved.

It is obvious that this bibasic acid stands in the same relation to ethylene as the monobasic ethylsulphurous acid to ethyl.

The origin of the two acids is perfectly analogous, the latter acid, according to Mr. Muspratt, being obtainable also by the action of nitric acid upon sulphocyanide of ethyl.

Sulphocyanide of ethyl, $C_4\,H_5\,Cy\,S_2$, produces ethylsulphurous acid, $C_4\,H_5,\ H,\ S_2\,O_6$.

Sulphocyanide of ethylene, $C_4\,H_4\,Cy_2\,S_4$, produces ethylensulphurous acid, $C_4\,H_4,\ 2H,\ 2S_2\,O_6$.

This reaction appears to throw some light upon the constitution of polybasic compounds. The compounds of monatomic molecules of the hydrogen-group with elements or compound radicals of the oxygen-group, are all remarkable for the simplicity of their construc-

tion. The union of biatomic radicals of the hydrogen-group with molecules of the oxygen-group gives rise to combinations of a far more complicated character. Whilst one molecule of water, $H_2\,O_2$, most conveniently may be considered as the type of many compounds of the former class, the corresponding compounds of biatomic radicals frequently correspond to a double molecule of water, $2H_2\,O_2$.

Sulphocyanide of potassium may be viewed as water, in which the oxygen is replaced by sulphur, one of the hydrogen molecules by cyanogen, the other by potassium.

$$\text{Water}\ \left.\begin{matrix}H\\H\end{matrix}\right\}O_2 \qquad \text{Sulphocyanide of potassium}\ \left.\begin{matrix}Cy\\K\end{matrix}\right\}S_2$$

In the production of sulphocyanide of ethylene two equivalents of chlorine in chloride of ethylene ($C_4\,H_4\,Cl_2$) have to be eliminated by two equivalents of potassium. Thus the very reaction of the two factors, chloride of ethylene ($C_4\,H_4\,Cl_2$), and two equivalents of sulphocyanide of potassium $2(K\,Cy\,S_2)$, joins 4 equivs. of sulphur and 2 equivs. of cyanogen with one molecule of ethylene. This reaction may be expressed by the following equation, which will illustrate at once my view in regard of the constitution of this substance:—

$$\begin{matrix}\left.\begin{matrix}Cy\\K\end{matrix}\right\}S_2\\ \left.\begin{matrix}K\\Cy\end{matrix}\right\}S_2\end{matrix} + C_4\,H_4\,Cl_2 = \begin{matrix}\left.\begin{matrix}Cy\\C_4\,H_4\end{matrix}\right\}S_2\\ \left.\begin{matrix}\\Cy\end{matrix}\right\}S_2\end{matrix} + 2K\,Cl.$$

The acid produced by the action of nitric acid upon sulphocyanide of ethylene obviously belongs to the same type. In this compound, which in the conception of this view may be called *ethylene-sulphurous* acid, the cyanogen is replaced by hydrogen, whilst the sulphur has been oxidized into the compound radical $S_2\,O_6$, which in sulphurous acid we assume united with hydrogen.

$$\begin{matrix}\text{Two}\\\text{mole-}\\\text{cules of}\\\text{water.}\end{matrix}\begin{matrix}\left.\begin{matrix}H\\H\end{matrix}\right\}O_2\\ \left.\begin{matrix}H\\H\end{matrix}\right\}O_2\end{matrix} \qquad \begin{matrix}\text{Two equivs.}\\\text{of bisulphite}\\\text{of potas-}\\\text{sium.}\end{matrix}\begin{matrix}\left.\begin{matrix}H\\K\end{matrix}\right\}S_2\,O_6\\ \left.\begin{matrix}H\\K\end{matrix}\right\}S_2\,O_6\end{matrix} \qquad \begin{matrix}\text{Ethylene-}\\\text{sulphu-}\\\text{rous}\\\text{acid.}\end{matrix}\begin{matrix}\left.\begin{matrix}H\\C_4\,H_4\end{matrix}\right\}S_2\,O_6.\\ \left.\begin{matrix}\\H\end{matrix}\right\}S_2\,O_6.\end{matrix}$$

Since we find that the hydrogen-molecules in polybasic acids are replaceable by two or more molecules of different metals or radicals,—witness tartrate of potassium and sodium, oxalovinate of potassium,—the idea naturally suggests itself that the biatomic alcohol-forming radicals may be capable of uniting two molecules of different elements

or compounds of the oxygen-group. It is probable, for instance, that the ethionic acid, discovered by M. Magnus, may be such a compound, namely *ethylene-sulphuro-sulphurous acid.*

Disulphetholic acid.	$\left.\begin{array}{c}H\\ C_4H_4\\ H\end{array}\right\}\begin{array}{l}S_2O_6\\ S_2O_6\end{array}$	Ethionic acid.	$\left.\begin{array}{c}H\\ C_4H_4\\ H\end{array}\right\}\begin{array}{l}S_2O_6\\ S_2O_8\end{array}$

The following Table contains some of the known ethylene and succinyl compounds compared with the corresponding derivatives of the ethyl and propionyl series.

Compounds of the Alcohol-forming Radicals.

Ethyl-series.		*Ethylene-series.*	
Ethyl	$\left.\begin{array}{c}C_4H_5\\ C_4H_5\end{array}\right\}$	Ethylene	C_4H_4
Chloride of ethyl	C_4H_5Cl	Chloride of ethylene..	$C_4H_4Cl_2$
Sulphide of ethyl	$\left.\begin{array}{c}C_4H_5\\ C_4H_5\end{array}\right\}S_2$	Sulphide of ethylene..	$C_4H_4S_2$
Mercaptan	$\left.\begin{array}{c}C_4H_5\\ H\end{array}\right\}S_2$	Ethylene-mercaptan..	$\left.\begin{array}{c}H\\ C_4H_4\\ H\end{array}\right\}\begin{array}{l}S_2\\ S_2\end{array}$
Sulphocyanide of ethyl.	$\left.\begin{array}{c}C_4H_5\\ Cy\end{array}\right\}S_2$	Sulphocyanide of ethylene.	$\left.\begin{array}{c}Cy\\ C_4H_4\\ Cy\end{array}\right\}\begin{array}{l}S_2\\ S_2\end{array}$
Bisulphide of ethyl.	$C_4H_5S_2$	Bisulphide of ethylene	$C_4H_4S_4$
Ethylsulphurous acid.	$\left.\begin{array}{c}C_4H_5\\ H\end{array}\right\}S_2O_6$	Ethylene-sulphurous acid.	$\left.\begin{array}{c}H\\ C_4H_4\\ H\end{array}\right\}\begin{array}{l}S_2O_6\\ S_2O_6\end{array}$
————————		Ethylene-sulphuro-sulphurous acid.	$\left.\begin{array}{c}H\\ C_4H_4\\ H\end{array}\right\}\begin{array}{l}S_2O_6\\ S_2O_8\end{array}$
Sulphovinic acid	$\left.\begin{array}{c}C_4H_5\\ H\end{array}\right\}S_2O_8$		

Compounds of the Acid-forming Radicals.

Propionyl-series.		*Succinyl-series.*	
Chloride of propionyl.	$C_2(C_4H_5)O_2, Cl$	Chloride of succinyl.	$C_4(C_4H_4)O_4, Cl_2$

Propionyl-series.		*Succinyl-series.*	
Propionic acid. .	$C_2(C_4H_5)\left.\begin{matrix}O_2\\H\end{matrix}\right\}O_2$	Succinic acid. .	$C_4(C_4H_4)O_4\left\{\begin{matrix}\left.H\right\}O_2\\\left.H\right\}O_2\end{matrix}\right.$
Anhydrous propionic acid.	$\left.\begin{matrix}C_2(C_4H_5)O_2\\C_2(C_4H_5)O_2\end{matrix}\right\}O_2$	Anhydrous succinic acid.	$C_4(C_4H_4)O_4, O_2$
Propionylamide	$N\left\{\begin{matrix}H\\H\\C_2(C_4H_5)O_2\end{matrix}\right.$	Succinamide.	$\left.\begin{matrix}N\left\{\begin{matrix}H\\H\end{matrix}\right.\\N\left\{\begin{matrix}H\\H\end{matrix}\right.\end{matrix}\right\}C_4(C_4H_4)O_4$

VII. "Description of an Instrument for registering Changes of Temperature." By Balfour Stewart, Esq. Communicated by J. P. Gassiot, Esq., F.R.S., Chairman of the Kew Observatory Committee of the British Association. Received June 12, 1856.

It lately occurred to the author that advantage might be taken of the capillary action of mercury to construct an instrument similar to a thermometer, but in which the mercury should expand from heat only in one tube, and contract from cold only in another. Accordingly a bulb was blown between two thermometric tubes of differently-sized bores, in such a manner that the tubes lay in one straight line, with the bulb between them. The bulb was then filled with mercury, and the tubes were hermetically sealed at both ends, having been first carefully deprived of air. When the instrument thus constructed was laid in a horizontal position, or nearly so, its action was precisely what the author had hoped; the mercury contracting from cold only in the narrow bore, and expanding from heat only in the wide one,—even when viewed by a microscope of considerable magnifying power.

It was suggested by Mr. Welsh, Director of the Kew Observatory, that such an instrument might be used for measuring fluctuations of atmospheric temperature; and the following use afterwards occurred to the author. Were it required to exactly estimate the radiating

effect of a source of heat, it might perhaps be done by placing this instrument near the source, alternately exposing it to the influence of the calorific rays proceeding from the source, and intercepting these by means of a screen. Owing to the peculiar action of the mercury alluded to, the effect of the rays would be multiplied by the number of times the screen was interposed, provided it were always retained long enough to permit the mercury to cool down. The comparison of an instrument thus acted upon with another similar instrument near it, screened entirely from the source of heat, might furnish us with a means of exactly estimating the heating effect of the source.

The author desires to express his obligation to Mr. Welsh, who, besides finding a use for the instrument, suggested the selection of tubes which appears to answer best, and whose experience was of great assistance in arranging details. He is also indebted to the Kew Committee of the British Association, who kindly examined the instrument, and authorized the construction of several by way of trial. Mr. Casella undertook the operative part in their construction, and his glass-blower, Mr. J. E. Griffin, took pains to discover some of the circumstances that interfere with the proper action of the instrument, and constructed those that have proved successful.

Without attempting to explain all the peculiarities of this action, it would seem that the mercury is kept in the narrow bore, and prevented from retreating into the bulb, by friction; but, when a moving force is supplied by means of a change of temperature, the motion of the mercury takes place in that direction in which it is least opposed, or most aided, by its capillary action.

As the result obtained is due to the difference between two forces, neither of which is very great, the construction of such an instrument requires care; and the author will now state what appear to be the chief points which demand attention, as derived from his own experience, and that of those who have interested themselves in the construction of the tubes; although this experience is necessarily very limited.

1st. The tubes should be quite clean and free from moisture.

2nd. They should be in one straight line, and should expand symmetrically into the bulb.

3rd. It seems the best arrangement, to have the narrow tube of flat bore, not too flat; its greatest width being about equal to the

diameter of the wide bore, which should be cylindrical, and neither conical nor flat.

4th. The tubes should be well deprived of air before being sealed. The instrument may be thus graduated.

If, when held vertically, the smaller tube being below, the mercury at the ordinary temperature should fill the lower tube, the bulb and part of the upper tube, the instrument may be pointed off in the same manner as an ordinary thermometer. But, if the mercury under these circumstances be not enough to fill the bulb, the best plan is perhaps to lay the instrument horizontally in a vessel of water, side by side with a standard thermometer, and, keeping the extremity of the mercury in the one tube at a constant point, to mark off its extremity in the other tube at two or more different temperatures, as shown by the standard thermometer. The length of this tube corresponding to a degree may be then found in the usual way.

The same process may be followed with the other tube. Or, take two points in the first tube—say A and B, the distance between them being, say 50°. Set the mercury at the point A, and mark off its other extremity in the second tube. Set it now at the point B, and mark its extremity in the second tube. The distance between these two points in the second tube will be the length corresponding to 50°.

Graduate the tubes to within a short distance of the bulb, and the best plan is perhaps to number the degrees from one extremity of the instrument, beginning 0...10...20, &c., on to the mark on that side nearest the bulb. Suppose this mark is numbered 100°; then number the mark nearest the bulb on the other side also 100°, and go on upwards numbering 110°, 120°, 130°, &c., until reaching the other extremity of the instrument. In the next place ascertain the temperature of the mercury when it fills the bulb and reaches only to the nearest mark on both sides.

Let this be C°. In taking an observation, note the numbers at both extremities of the mercury, and deduct the less from the greater. To the *positive* remainder add the constant C *with its proper sign*, and the sum will give the true temperature.

VIII. "Researches on Organo-metallic Bodies." Third Memoir. —On a New Series of Organic Acids containing Nitrogen. By EDWARD FRANKLAND, Ph.D., F.R.S. Received June 19, 1856.

(Abstract.)

The author, in pursuing the line of research indicated in his former memoirs, has investigated the action of zincethyl and zincmethyl upon binoxide of nitrogen, and has succeeded in producing a new series of organic acids, by the substitution of oxygen in binoxide of nitrogen by methyl, ethyl, &c. Binoxide of nitrogen is slowly absorbed by zincethyl, and the sole product of the reaction is a body which is deposited in magnificent rhomboidal colourless crystals. This body has the composition expressed by the formula

$$N_2 C_4 H_5 O_4 Zn + C_4 H_5 Zn,$$

and consists of the zinc salt of a new acid, for which the author proposes the name *dinitroethylic acid*, united with zincethyl. The dinitroethylate of zinc and zincethyl is produced from binoxide of nitrogen and zincethyl according to the following equation,

$$\left.\begin{matrix} 2(Zn C_4 H_5) \\ 2NO_2 \end{matrix}\right\} = N_2 C_4 H_5 O_4 Zn + C_4 H_5 Zn.$$

The crystals of dinitroethylate of zinc and zincethyl instantly become opake on exposure to the air, owing to the formation of an oxidized product. They are tolerably soluble in anhydrous ether without decomposition, but are instantly decomposed by anhydrous alcohol and by water. Exposed to the gradually increasing heat of an oil-bath, dinitroethylate of zinc and zincethyl fuses at 100° Cent., froths up, and begins slowly to evolve gas. At 180° the colour darkens, a small quantity of a highly alkaline liquid distils over, and a large amount of gas is evolved. The latter consists of carbonic acid, olefiant gas, hydride of ethyl, nitrogen and protoxide of nitrogen. When brought into contact with water, dinitroethylate of zinc and zincethyl is immediately decomposed with lively effervescence, due to the evolution of pure hydride of ethyl. An opalescent solution is formed, possessing a powerfully alkaline reaction and a

peculiar bitter taste. The opalescent solution contains only basic dinitroethylate of zinc, and the reaction is expressed by the following equation:—

$$N_2\,C_4\,H_5\,O_4\,Zn + \left.\begin{matrix} C_4\,H_5\,Zn \\ HO \end{matrix}\right\} = \left\{\begin{matrix} C_4\,H_5,\ H \\ N_2\,C_4\,H_5\,O_4\,Zn + Zn\,O. \end{matrix}\right.$$

Carbonic acid decomposes this basic salt, precipitating carbonate of zinc, and leaving the neutral salt in solution.

Dinitroethylate of zinc and zincethyl is also decomposed by dry oxygen according to the following equation:—

$$N_2\,C_4\,H_5\,O_4\,Zn + \left.\begin{matrix} C_4\,H_5\,Zn \\ O_2 \end{matrix}\right\} = \left\{\begin{matrix} N_2\,C_4\,H_5\,O_4\,Zn \\ ZnO\,C_4\,H_5\,O. \end{matrix}\right.$$

When the product of oxidation is treated with water, basic dinitroethylate of zinc is produced along with alcohol.

Neutral dinitroethylate of zinc crystallizes in minute colourless needles containing half an equivalent of water. It fuses at 100° Cent., and gradually becomes anhydrous. It is very soluble in water and in alcohol. Heated suddenly in air to about 300° Cent. it burns rapidly with a bluish green flame.

Dinitroethylic acid can only exist in dilute solution; it can be prepared, either by decomposing the zinc salt with dilute sulphuric acid and distilling *in vacuo*, or by decomposing the baryta salt by an exact equivalent of dilute sulphuric acid. The dilute acid thus prepared possesses a pungent odour, somewhat resembling that of the nitro-fatty acids, and an acid taste. It reddens litmus paper strongly, and gradually decomposes even at ordinary temperatures. Neutralized by the carbonates of the various bases, it yields the corresponding salts. The silver and magnesian salts thus prepared were analysed.

The salts of dinitroethylic acid are all soluble in water and in alcohol, and most of them crystallize with more or less difficulty. They are all violently acted upon by concentrated nitric acid, the dinitroethylic acid being entirely decomposed and a nitrate of the constituent base produced. Dilute nitric acid acts in the same manner, but more slowly. They all fuse at a temperature little above 100° Cent. The potash, soda, lime, and baryta salts deflagrate explosively, like loose gunpowder, at a temperature considerably below redness.

The following salts have been prepared and analysed :—

	Formulæ.
Dinitroethylate of silver	$N_2 C_4 H_5 O_4 Ag$
Double nitrate and dinitroethylate of silver	$N_2 C_4 H_5 O_4 Ag + NO_6 Ag$
Dinitroethylate of copper	$2(N_2 C_4 H_5 O_4 Cu) + HO$
Dinitroethylate of zinc (crystallized)	$2(N_2 C_4 H_5 O_4 Zn) + HO$
Dinitroethylate of zinc (anhydrous)	$N_2 C_4 H_5 O_4 Zn$
Dinitroethylate of zinc (basic)	$N_2 C_4 H_5 O_4 Zn + ZnO$
Dinitroethylate of zinc and zincethyl	$N_2 C_4 H_5 O_4 Zn + C_4 H_5 Zn$
Dinitroethylate of baryta	$N_2 C_4 H_5 O_4 Ba$
Dinitroethylate of lime	$N_2 C_4 H_5 O_4 Ca + 3HO$
Dinitroethylate of magnesia	$N_2 C_4 H_5 O_4 Mg$
Dinitroethylate of soda	$N_2 C_4 H_5 O_4 Na$.

Dinitromethylic acid.—When binoxide of nitrogen is absorbed by zincmethyl, dinitromethylic acid is produced, and forms a series of salts homologous with those of dinitroethylic acid. The formula of this acid is

$$N_2 C_2 H_3 O_4 H,$$

and the following salts have been examined :—

Dinitromethylate of zinc	$N_2 C_2 H_3 O_4 Zn + HO$
Dinitromethylate of soda	$N_2 C_2 H_3 O_4 Na + 2HO$
Dinitromethylate of zinc and zincmethyl	$N_2 C_2 H_3 O_4 Zn + C_2 H_3 Zn$?

It is difficult to arrive at any satisfactory conclusion relative to the rational constitution of this series of acids; they may be regarded as belonging to the type of nitrous acid, containing a double equivalent of nitrogen, and in which one atom of oxygen has been replaced by an alcohol radical, thus,

$$N_2 \begin{cases} Cn\,Hn+1 \\ O \\ O \\ O \end{cases}$$

or they may be viewed as constructed upon the hyponitrous acid type, one equivalent of oxygen being replaced by an alcohol radical, and a second by binoxide of nitrogen, thus,

$$N \begin{cases} Cn\,Hn+1 \\ NO_2 \\ O \end{cases}$$

Without attaching much value to either hypothesis, the author prefers the latter, and remarks in conclusion that there can be little doubt that many new series of organic acids may, by analogous processes, be produced from inorganic acids by the replacement of one or more atoms of oxygen by an alcohol radical; in fact his pupil, Mr. Hobson, is now engaged in the study of a new series containing sulphur, produced by the action of zincethyl and its homologues upon sulphurous acid. These acids are formed by the replacement of one equivalent of oxygen, in three equivalents of sulphurous acid, by an alcohol radical.

IX. "On the Action of Urari and of Strychnia on the Animal Economy." By Professor Albert Kölliker of Würzburg. Communicated by Dr. Sharpey, Sec. R.S. Received May 31, 1856.

The communication which I now offer to the Royal Society contains a brief statement of the results of a series of experiments which I lately made on the action of the urari poison and of strychnia on the animal economy.

I. Urari.

The urari is the well-known poison from Guiana, also called Curare and Woorara. That which I employed in my experiments I owe to the liberality of my friend Professor Christison of Edinburgh. The following are the conclusions at which I arrived respecting its operation:—

1. The urari causes death very rapidly when injected into the blood or inserted into a wound; when introduced by way of the mucous membrane of the intestinal canal its effects are slow and require a large dose for their production, especially in mammalia. When applied to the skin of frogs it is altogether inoperative.

2. Frogs poisoned with very small doses of urari may gradually recover, even after it has produced complete paralysis of the nerves. Mammalia may also be restored, even after large doses, provided respiration is maintained artificially.

3. The urari, *acting through the blood, destroys the excitability*

of the motor nerves. In frogs under its operation the terminal branches of these nerves within the muscles lose their excitability in a few minutes, whilst their trunks become affected an hour or two later. If, after the nervous extremities have become paralysed, the heart of the animal be excised so as to prevent the nerves from receiving any further share of the poison, the nervous trunks may retain their excitability for three or four hours.

4. The brain is less affected by the urari than the nerves in the muscles; still when, by ligature of the two aortic arches, in frogs, the poisoning is confined to the anterior half of the body, the voluntary movements of the limbs speedily cease, whilst automatic movements, of doubtful nature and probably proceeding from the medulla oblongata, may be still observed for half an hour or an hour after the poison has begun to operate.

5. The spinal cord is considerably less affected than the brain by this poison, and by local limitation of the poisoning (as in No. 4), it is found that the cord retains its *reflex activity* from half an hour to an hour and a half, and the excitability of its white substance or its conducting power from two to three hours after the poison has taken effect. It is worthy of remark that in such cases the impaired reflex activity of the spinal cord may be revived by strychnia directly applied to it.

6. The *sensory nerves,* as shown also by locally limited poisoning, retain their functional activity as long at any rate as reflex actions can be excited, and when the depressed reflex activity has been revived by means of strychnia, these nerves are found not to have been in the slightest degree injured, so that it seems doubtful whether the urari in any way affects them.

7. *The nerves of the involuntary muscles and of the glands* are also paralysed by the action of urari, at least I find this to be true in the following cases, viz.—

a. The pneumogastric, as regards its influence on the heart.

b. The sympathetic (its cervical portion), in its relation to the iris.

c. The nerves of the posterior lymph-hearts of the frog.

d. The nerves of the vessels in the web of the frog's foot.

e. The splanchnic nerves of the rabbit, as affecting the peristaltic motions.

f. The nerves governing the secretion of the submaxillary gland in dogs.

8. *The voluntary muscles* remain perfectly excitable, but show a greater tendency than usual to merely local contractions. In general the cadaveric rigidity of these muscles appears to set in later than usual.

9. *The plain or non-striated muscles* also remain long irritable after poisoning by urari.

10. The heart, in amphibia, is little affected by urari. Its pulsation as well as the circulation of the blood goes on regularly for many hours after the poisoning is established. The only thing worthy of note is that the beat of the heart appears to be somewhat quickened, probably from paralysis of the pneumogastric nerves. In frogs poisoned with urari, the heart, when cut in two, shows the usual phenomenon, namely, that the half which contains the ganglia continues to pulsate whilst the other does not; from which it may be inferred that these ganglia are not paralysed. As to the nerves in the substance of the heart, those at least which are derived from the pneumogastric are unquestionably paralysed (vide No. 7).

11. The lymph-hearts of frogs poisoned with urari soon cease to move.

12. The blood of animals poisoned by urari is fluid and dark, but coagulates when drawn from the vessels, and forms a weak clot which is but little reddened by exposure to air. Directly mixed with blood, urari does not prevent coagulation, but the blood in this case also remains dark and scarcely reddens on exposure.

13. The blood of animals poisoned by urari has the same poisonous qualities as that substance itself, but not in a degree sufficient to produce the full effects of the poison. Urari when directly mixed with blood loses none of its efficacy.

14. Urari, in concentrated solution, applied locally to nerves extinguishes their excitability, but only after a considerable time, and it appears to act similarly on the nerves in the substance of the muscles. Dilute solutions have no injurious operation. Applied directly to the brain and spinal cord, urari is altogether harmless provided its absorption be prevented.

15. When artificial respiration is kept up in quadrupeds poisoned with urari, I find that, as observed by Bernard, many of the secre-

tions become increased—as the tears, saliva, urine and mucus of the air-passages, which effect appears to be owing to the paralysis of the vascular nerves and consequent dilatation of the vessels caused by the poison.

16. In mammalia urari causes death by paralysis of the respiratory nerves and suppression of the respiration, which brings on convulsions in these animals as a collateral effect. In frogs the final extinction of the functions may also be partly ascribed to suppressed action of the lungs and defective oxidation of the blood, which at length renders the heart unfit to perform its office; but it must be observed that in this case the cause of death is not so plain, inasmuch as in these animals the functions are in a great degree independent of the pulmonary respiration.

II. Strychnia.

Some experiments with strychnia (the acetate) gave the following results:—

1. Strychnia has not the least influence on the peripheral nerves through the blood, which is best shown by cutting the nerves before administering the poison.

2. Strychnia paralyses the motor nerves of the voluntary muscles by exciting them to too energetic action, a paralysis which may be compared to that caused by powerful electric currents acting upon the nerves. In frogs, when the tetanic spasms are over, the nerves often show no trace of excitability; in mammalia they generally retain it in a slight degree, but never show the same energy of action as when uninjured.

3. Strychnia does not affect the sensory nerves.

4. The heart is not affected by strychnia, not even during the tetanic spasms, with the exception only that its pulsations are sometimes a little slower during the tetanic state. On the contrary, the lymph-hearts of frogs contract themselves as soon as the tetanus begins, and remain in this state as long as the spasms last.

5. The tetanic fits can be brought on in two ways; first, through *the sensory nerves*, which, by irritating the grey substance of the spinal cord, produce the tetanic contractions as reflex movements; and, secondly, *through the brain*, which is not affected at all by strychnia and preserves its powers of volition and sensation. Accord-

ingly, animals poisoned with strychnia try to move in the ordinary way, but every attempt brings on a tetanic fit, so that it is plain that the spinal cord may also be excited by the brain to its peculiar actions.

6. If the tetanus produced by strychnia has been strong, the *muscles are less irritable* and pass much sooner into the state of *cadaveric rigidity*, which is very strongly marked, and seems to last longer than it generally does. The same early onset of rigidity may be observed in animals killed by tetanus excited by electricity.

X. "Researches on the Foraminifera."—Part II. By WILLIAM B. CARPENTER, M.D., F.R.S., F.G.S. &c. Received June 19, 1856.

(Abstract.)

In the pursuance of his plan of minutely examining certain typical forms of Foraminifera, for the purpose of elucidating their history as living beings, and of determining the value of the characters they present to the systematist, the author in this memoir details the results of his investigations on the genera *Orbiculina, Alveolina, Cycloclypeus*, and *Heterostegina*.

The genus *Orbiculina* has long been known, through its prevalence in the West Indian seas, which causes its shells to abound in the shore-sands of many of the islands of that region. These shells present great varieties of form, and have been ranked under three distinct species; but M. d'Orbigny has correctly inferred, from a comparison of a large number of specimens, that their diversities of form are partly attributable to differences in the stage of growth, and partly to individual variation, so that all the *Orbiculinæ* of Cuba, the Antilles, &c., are referable to but one specific type. Of the essential features of its structure, however, he would seem to be quite ignorant; since he ranges *Orbiculina* in a distinct order from *Orbitolites*, to which it is very closely allied. This alliance was first pointed out by Prof. Williamson, whose account of the structure of Orbiculina, though defective and erroneous in certain points, is nevertheless correct in the main.

The author has had the opportunity of examining not merely a

considerable number of West Indian specimens, but also a set of specimens peculiarly remarkable for their high development, which form part of Mr. Cuming's Philippine collection. Many of these present the form of flattened disks, marked with concentric circles, and having one or more rows of pores at their edges, not distinguishable, save by their prominent central nuclei, from certain forms of Orbitolites formerly described. The similarity is equally great in their internal structure; so that, if a marginal fragment only were submitted to examination, it would not be possible to say with certainty whether it belonged to an *Orbitolites* or an *Orbiculina.* The distinguishing character of the latter is derived from its early mode of growth, which is uniformly *spiral*; and from the circumstance that each of the first three or four turns of the spire not merely surrounds, but invests its predecessor, thereby producing an excess in the thickness of the earlier over that of the later-formed portion, which gives rise to the central protuberance already mentioned. The transition from the spiral to the cyclical mode of increase is effected (just as it is in those individuals of Orbitolites which begin life upon the spiral type) by the opening-out of the mouth of the spire, which extends itself on either side around the previously-formed body, until its two divisions meet on the opposite side, where they coalesce so as to constitute a complete annulus. This transition may take place at any period of growth after the completion of the first four or five turns of the spire; so that we sometimes meet with small specimens which have already become discoidal and taken-on the cyclical plan of growth, whilst we occasionally meet with full-grown specimens which retain the spiral form, and show no tendency whatever towards the assumption of the cyclical plan of growth. These facts obviously point to the very subordinate value of *plan of growth* as a distinctive character.

The author next proceeds to a like investigation of the genus *Alveolina*, which he shows to bear a very marked resemblance to Orbitolites and Orbiculina, in the simple concretionary texture of the shell, in the freedom of communication everywhere existing among the chambers, in the mutual relations of these to each other, and in their mode of communication with the exterior; whilst its plan of growth is very different, the axis round which the spiral turns being greatly elongated, and every additional whorl of the spire pro-

ducing a much greater augmentation of its length than of its diameter. There is obviously a close *physiological* relationship between this genus and the preceding, since the condition of each individual segment of the sarcode-body must be essentially the same in each; and it is merely in the mode in which these segments are multiplied,—a character which we have seen not to be constant in different parts even of the same specimens of Orbitolites and Orbiculina,—that it differs from them.

A marked contrast to Orbitolites and Orbiculina in all their physiological characters, coexisting with an agreement in their respective plans of growth, is presented by the genera *Cycloclypeus* and *Heterostegina*; the former of which, like Orbitolites, is cyclical from the beginning, its chambers being formed in successive annuli round a central cell; whilst the latter, like Orbiculina, is spiral in the first instance, but tends, as age advances, to assume the discoidal shape and cyclical plan of growth. The genus *Cycloclypeus* is a new one, founded by the author upon specimens dredged-up by Sir E. Belcher off the coast of Borneo. These are the largest Foraminifera at present known to exist; the diameter of some of them being not less than 2¼ inches. The genus *Heterostegina* was formed by M. d'Orbigny; but he seems only to have been acquainted with young specimens, and has altogether misapprehended its true characters and relations. A fragment of the flattened spire of Heterostegina could scarcely be distinguished from a marginal portion of the disk of Cycloclypeus; so close is the conformity between the two, as regards the form and relations of the chambers, their mode of communication, and the structure of their shelly envelope. Each chamber, as in Nummulites, has its own proper wall, so that the partition between the adjacent chambers, whether of the same row or of different rows, is double; and between its two lamellæ there is interposed an additional stratum of shell that belongs to neither. This additional stratum is thin, in the septa dividing adjacent chambers of the same row; but it is much thicker, and forms a much more complete separation, in the septa intervening between different rows. It is traversed by a canal-system, analogous to that existing in Nummulites; which the author believes to be occupied in the living state by threads of sarcode, and to be specially destined for the nutrition of the 'intermediate skeleton' formed by the aggregate of these inter-

posed lamellæ. The chamber is covered-in above and below by successive layers of a minutely-tubular and peculiarly-compact shell-substance, resembling dentine in its general aspect; certain parts of this, however, are non-tubular, and form cones, of which the bases appear on the surface as minute rounded tubercles. The adjacent chambers of the same row do not seem to communicate with each other; but each chamber communicates with two chambers of the previously-formed row, and, in like manner, with two of the subsequently-formed row, by narrow passages, the number and position of which are by no means constant. These passages seem to afford the principal means whereby the segments of the sarcode-body occupying the inner chambers, can be nourished from the exterior; but it is by no means impossible that the tubuli of the shelly laminæ that invest the chambers above and below, may also be subservient to this purpose, since, however numerous may be the laminæ, the tubuli are continued through them all from the cavity of the chamber to the external surface.

The almost entire separation of the segments of the sarcode-body in these two genera, the investment of each of them with its own proper envelope of shell, the minutely-tubular structure and firm consistence of the shell-substance, and the interposition of the intermediate skeleton with its canal-system, are features that place them in such marked contrast with Orbitolites and Orbiculina, that, notwithstanding their conformity to those two genera in their respective plans of growth, it is scarcely possible for them to be more widely removed in everything that relates to their respective physiological conditions.

From a comparison of the five genera whose structure has been thus elucidated, the author deduces the conclusion that, in this class, external form, which depends exclusively on plan of growth, affords no clue whatever to internal structure; and that the latter alone, as the exponent of the physiological condition of the animal, can afford the basis of a natural classification.

XI. "Electro-Physiological Researches."—Tenth Series. Part I. By Signor CARLO MATTEUCCI, Professor in the University of Pisa. Communicated by MICHAEL FARADAY, D.C.L., F.R.S. &c. Received June 12, 1856.

(Abstract.)

In the first section of this part of his Memoir, Professor Matteucci treats of the heat developed by muscles during contraction. Adverting to earlier experiments by other inquirers, in which a rise of temperature was observed to take place in the contracting muscle while the blood still continued to circulate through it, and where, consequently, it remained uncertain how far the effect might not be due to a modification of the circulation, the author describes his own experiments, by which it is shown that the muscles of frogs, after all circulation of the blood has ceased, and by the sole act of contraction, cause a rise of temperature amounting to about half a degree Centigrade.

The second section is devoted to the consideration of the electric current exhibited by muscles at rest, and experiments are adduced in illustration of the following propositions:—

a. The electro-motive power of a cut muscle is independent of the size of its transverse section.

b. The electro-motive power increases with the length of the muscle.

c. The electro-motive property of the muscles of living or recently killed animals is greater in mammals and birds than in fish and amphibia. The *duration* of this force, which in all cases decreases most rapidly in the first moments after death, is greater in fish and amphibia than in the higher orders of animals.

d. The nerves have no direct influence on the electro-motive force of muscles. In general, all causes which exert an influence on the physical structure and chemical composition of muscles, so as to modify, in ways unknown, their irritability or contractility, act equally on their electro-motive power.

Prof. Matteucci here takes occasion to state, that he has verified the important discovery of Du Bois Reymond, of the existence of an

electric current in portions of nerve detached from the body,—a current which, like that of muscle, passes, in the galvanometer, from the surface of the nerve to the interior. He adds, that he could perceive no marked difference in the relative duration of the electromotive power of muscle and nerve.

In the third section, the author discusses anew, and with the aid of fresh experiments, the phenomenon originally named by him *induced contraction,* in which the nerve of a "galvanoscopic limb" of a frog, being laid on a muscle of a living or recently killed animal, is stimulated by the contraction of that muscle, so as to cause at the same moment contraction in the muscles of the galvanoscopic limb. He endeavours to show, that this phenomenon is due to an actual electric discharge, which takes place in a muscle at the moment of contraction, in an opposite direction to the ordinary current of the muscle while at rest. The deviation of the galvanometer-needle during the contraction of a muscle, which occurs in an opposite direction to that previously caused by the electric current generated by the muscle while at rest, was ascribed by M. Du Bois Reymond to the diminution or cessation of the latter current when the muscle contracts, and to the operation in such circumstances of the secondary polarity of the platinum plates of the galvanometer. Professor Matteucci, however, adduces various experiments to show, that, by certain arrangements described, he is able entirely to prevent the occurrence of secondary polarity, and that nevertheless the deviation of the needle takes place.

After adverting to the want of reliable data on which to found an explanation of the physical cause of the phenomenon in question, the author hints, that as there is no analogy between the form of the voltaic electromotor and that of the molecular electromotor, it is not impossible to conceive that the change of form which takes place in a muscle during contraction may be momentarily followed by the inversion of the muscular current in the exterior arc. He observes, that examples are not wanting, taken from certain cases of electrodynamic induction, and also of voltaic circuits, in which this inversion of the current can be obtained by a change in the form, or relative distance of different parts of the circuit; but he adds, that this is a new field of inquiry, which cannot be given up to merely hypothetical views.

The last section of the paper is devoted to the consideration of the mechanical effects of muscular contraction; and experiments are stated with a view to compare the *effective work* of a muscular contraction, as determined empirically with the work calculated according to the principles of the dynamical theory of heat.

Employing the dynamometer already described in the fourth series of his researches, the author has found that the mechanical work effected by a single contraction of the gastrocnemius muscle of a frog may be expressed by 0·00001457 kilogramme-metres. Determining next the quantity of zinc required to be oxidated in the pile in order to excite a single contraction, he finds that the force developed by the muscle is enormously greater than could be accounted for on the supposition that it is produced by the conversion into muscular energy of the equivalent of electricity corresponding to the quantity of zinc oxidated. He accordingly concludes, that the electric current which *excites* a muscle to action does not represent the force exerted by the muscle, which is more probably to be referred to the chemical changes, such as oxidation, which take place in the muscular tissue during contraction. The consideration of this branch of the inquiry will form the subject of the second part of the Memoir.

XII. "On the Existence of Multiple Proportion in the quantities of Heat produced by the Chemical Combination of Oxygen and other bodies." By Thomas Woods, M.D. Communicated by Professor Stokes, Sec. R.S. Received June 7, 1856.

(Abstract.)

This paper is, in substance, the same as a former paper, bearing a similar title, read before the Royal Society on the 10th January 1856, but contains a more detailed account of the mode of performing the experiments. A repetition of the experiments mentioned in the former paper has led to very nearly the same numerical results, except in the case of molybdenum, which is found to give 4·8 thermal units by combining with oxygen, instead of 3·38, the number formerly given.

XIII. "Researches into the nature of the Involuntary Muscular Fibre." By GEORGE VINER ELLIS, Esq., Professor of Anatomy in University College, London. Communicated by Dr. SHARPEY, Sec. R.S. Received June 11, 1856.

(Abstract.)

Having been unable to confirm the statements of Professor Kölliker respecting the cell-structure of the involuntary muscular fibre, the author was induced to undertake a series of researches into the nature of that tissue, by which he has been led to entertain views as to its structure in vertebrate animals, but more especially in man, which are at variance with those now generally received. The present communication contains the results of these inquiries, which tend to show that the voluntary and involuntary muscles resemble each other very closely in the arrangement and constitution of their fibres.

After adverting to the present state of opinion on the subject, the author gives an account of his own observations, and treats successively of the interweaving of the fibres, their size, form, and ultimate structure; their mode of attachment at their extremities, their length, and the corpuscles connected with them. He devotes a section also to the question of the periodic formation and destruction of muscular fibres in the uterus, in its different conditions; and while he is led by his own investigations to recognize an enlargement in size of the individual fibres of that organ during pregnancy, followed by subsequent diminution, he is unable to confirm the doctrine of new formation. Moreover, he finds that during pregnancy a considerable amount of granular matter, with round or oval granular-cells, is deposited among the fibres. He adduces reasons for believing that this substance cannot be regarded as a blastema, nor its imbedded cells as formative cells, for the production of new fibres; and he is disposed to ascribe the enlargement of the uterus in pregnancy principally to the enlargement of the muscular fibres, and the addition of this new deposit.

The following is a summary of the conclusions which the author has arrived at on the main subject of his inquiry :—

In both kinds of muscles, voluntary and involuntary, there is an interweaving of the fibres with the formation of meshes.

The fibres in both kinds are long, slender, rounded cords of uniform width, except at the ends, where they are fixed by tendinous tissue; and in both, the size of the fibres in the same bundle varies greatly.

In neither voluntary nor involuntary muscle is the fibre of the nature of a cell, but in both is composed of minute threads or fibrils. Its surface-appearance in both kinds of muscle allows of the supposition that in both it is constructed in a similar way, namely, of small particles or "sarcous elements," and that a difference in the arrangement of these elements gives a *dotted* appearance to the involuntary and a *transverse striation* to the voluntary fibres.

The length of the fibres varies in both cases with the organ or part examined, and the connexion with tendon always takes place after the same manner, whether the fibre is dotted or striated.

On the addition of acetic acid, fusiform or rod-shaped corpuscles make their appearance in all muscular tissue; these bodies, which appear to belong to the sheath of the fibre, approach nearest in their characters to the corpuscles belonging to the yellow or elastic fibres which pervade various other tissues; and, from the apparent identity in nature of these corpuscles in the different textures in which they are found, and especially in voluntary as compared with involuntary muscle, it is scarcely conceivable that in the latter case exclusively they should be the nuclei of oblong cells constituting the proper muscular tissue.

The paper concludes with a statement of the mode of procedure which the author has found most suitable for examining the tissue which forms the subject of his inquiry.

XIV. "Account of the Construction of a Standard Barometer, and of the Apparatus and Processes employed in the Verification of Barometers at the Kew Observatory." By JOHN WELSH. Communicated by J. P. GASSIOT, Esq., F.R.S., Chairman of the Kew Observatory Committee of the British Association.

(Abstract.)

After stating the results of experiments, made under the superintendence of the Kew Committee, for the construction of a barometer tube of large diameter by the usual method of boiling the mercury in the tube, the author proceeds to describe a method of filling a tube with the aid of an air-pump. In this process, which is fully detailed in the paper, the tube is so constructed, that when the air has been extracted from it, the mercury enters by atmospheric pressure, provision being made for entirely removing the air which the air-pump has failed to extract. By this method a barometer tube of 1·1 inch internal diameter has been satisfactorily prepared at the Kew Observatory. The author then describes the mounting and mode of observing the standard barometer, proceeds to explain the processes adopted in the verification of barometers, and gives a detailed description of the apparatus for determining the errors of barometers at different atmospheric pressures.

XV. "On the Aurora." By REUBEN PHILLIPS, Esq. Communicated by Professor STOKES, Sec. R.S. Received March 7, 1856.

In this paper the author enters into various speculations as to the formation and motion of auroral arches. Since it has been found by experiment that the maximum length of the voltaic arc with a given battery is nearly the same in atmospheric air and in highly rarefied air, forming a very perfect vacuum, the author conceives that a streamer begins as a disruptive discharge of finite and very moderate length, (the maximum length very nearly of a continuous discharge,) which starts upwards from the auroral arch, which he

regards as the discharging train. If this first portion be not parallel to the dipping-needle, it is moved laterally by virtue of the earth's magnetism, and thus wrenched, as it were, from the spot where it was formed, and extinguished. If, however, the discharge, or any portion of it, be parallel to the dipping-needle, it is not influenced by the earth's magnetism, and remains. To this first length another length may be added by a similar process, and so on, these successive lengths being all parallel to the direction of the first, since otherwise the streamer would be torn asunder by the lateral motion resulting from the earth's magnetism. Thus a straight streamer extends upwards in a direction parallel to the dipping-needle.

If, from some increase in the power of conduction of the arch, the base of the streamer be not necessarily confined to a single spot, then a streamer may be formed which is somewhat inclined to the dipping-needle; but the consecutive elements of such a streamer must be in the same direction, otherwise they would have different lateral motions, the streamer would be divided, and the discharge would cease. The streamer, as a whole, will move from east to west, or from west to east, according to circumstances. Those streamers which would tend to move north or south cannot exist, because their bases would be severed from the auroral arch.

If the discharge take place in air not so very highly rarefied, so that the disruptive discharge is not quite of its maximum length, consecutive elements need not be quite in the same direction in order that the streamer may be unbroken, and thus curved streamers may be formed. It is stated by M. Biot that such have sometimes been observed.

The author then enters into some speculations as to the nature of the auroral arch, which he conceives to consist of nebulous matter highly charged with electricity, and accounts, according to his views, for the motion of such arches from the pole towards the equator.

The remainder of the paper is occupied with speculations as to the nature of fire-ball lightning, and other subjects relating to ordinary electricity.

XVI. "On the Lunar-diurnal Magnetic Variation at Toronto." By Major-General Edward Sabine, R.A., D.C.L., Treas. & V.P.R.S. Received June 13, 1856.

(Abstract.)

This paper contains the results of an investigation into the moon's diurnal influence on the horizontal and vertical components of the magnetic force at Toronto, and the consequent deduction of the lunar-diurnal variations of the inclination and of the total force at that station. The observations from which the results were obtained consisted of five years of hourly observation of the bifilar and vertical force magnetometer, ending June 30, 1848, from which the disturbances of largest amount had been separated as described in a paper previously communicated (Phil. Trans. 1856, Art. XV.). The results derived from the mean of the five years are confirmed by the accord which is shown of the means of each of the half-periods into which the observations of the five years are divided for that purpose.

To complete the view of the moon's diurnal influence on the magnetic elements at Toronto, a recalculation has been made of the lunar-diurnal variation of the declination from the mean of *six* years of hourly observation, ending June 30, 1848, employing the more perfect normals derived from the exclusion of the larger disturbances, as described in the paper above referred to (Phil. Trans. Art. XV. 1856); and the confirmation is shown of the mean result of the six years by the accordance of three separate portions of two years each, into which the whole period of six years has been divided for that purpose.

From these premises the author draws the following conclusions:

1. The three magnetic elements concur in showing that the moon exercises a sensible magnetic influence at the surface of the earth, producing in every lunar day a variation which is distinctly appreciable, in each of the three elements, by the instruments adopted and recommended in the Report of the Committee of Physics of the Royal Society, when due care is taken in conducting the observations, and suitable methods are employed in elaborating the results.

2. That the lunar diurnal variation in each of the three elements constitutes a double progression in each lunar day; the declination having two easterly and two westerly maxima, and the inclination and total force each two maxima and two minima between two successive passages of the moon over the astronomical meridian; the variation passing in every case four times through zero in the lunar day. The approximate range of the lunar-diurnal variation at Toronto is 38″ in the declination, 4″·5 in the inclination, and ·000012 parts of the total force.

3. That the lunar-diurnal variation thus obtained appears to be consistent with the hypothesis that the moon's magnetism is, in great part at least if not wholly, derived by induction from the magnetism of the earth.

4. That there is no appearance in the lunar-diurnal variation of the *decennial* period, which constitutes so marked a feature in the solar diurnal variations.

XVII. "On Autopolar Polyedra." By the Rev. Thomas P. Kirkman, M.A. Communicated by Arthur Cayley, Esq., F.R.S. Received June 19, 1856.

(Abstract.)

An autopolar polyedron is such, that any type or description that can be given of it remains unaltered, when summits are put for faces, and faces for summits. To every β-gon B in it corresponds a β-ace b (or summit b of β edges), which may be called the pole of that β-gon; and to every edge AB, between the α-gon A and the β-gon B, corresponds an edge ab, between the α-ace a and the β-ace b. Two such edges are called a *gamic pair*, or *pair of gamics*.

The enumeration of autopolar p-edra is here entered upon as a step towards the determination of the number of p-edra. The theorems following are established, and shown to be of importance for the solution of the general problem.

Theorem I.—*No polyedron, not a pyramid, has every edge both in a triangle and in a triace.*

Def. An edge of a polyedron is said to *convanesce*, when its two summits run into one; and it is said to *evanesce*, when its two faces revolve into one.

An edge (AB) is said to be *convanescible*, when neither of the faces A and B is a triangle, and (AB) joins two summits which have not two collateral faces, one in either summit, besides A and B.

An edge (ab) is said to be *evanescible*, when neither a nor b is a triace, and the two faces about (ab) are not, one in either, in two collateral summits, besides a and b.

THEOREM II.—*Every polyedron, not a pyramid, has either a convanescible or an evanescible edge.*

THEOREM III.—*Any p-edral q-acron, not a pyramid, can be reduced by the vanishing of an edge, either to a* $(\mathrm{p}-1)$*-edral* q*-acron, or to a* p*-edral* $(\mathrm{q}-1)$*-acron.*

By such a reduction of a p-edral q-acron P to P′, of P′ to P″, &c., P can be shown to be *generable* from a certain pyramid Π; by which it is meant that Π is the highest-ranked pyramid to which P can by such reduction be reduced.

Hereby it is evident that the problem of enumeration of the x-edra is brought down to this: to *determine how many* $(\mathrm{r}+\mathrm{m})$*-edra are generable from the* r*-edral pyramid.*

The autopolars so generable are first considered, as the heteropolars are obtained by combination and selection of those operations with which the theory of the autopolars makes us acquainted.

Autopolarity is of three kinds, nodal, enodal, and utral.

Every even-based pyramid is nodally autopolar; *i. e.* it cannot but have two nodal summits. For example, the 5-edral and 7-edral pyramids have the signatures of their faces and summits thus arranged,—

1 2 3 4		1 2 3 4 5 6
4 3 2 1		6 5 4 3 2 1'

the upper line showing the triangles, and the lower the triaces about the base, which as well as its pole the vertex, is signed zero. The two triaces in the triangle 5 are 3 and 2; the two triangles in the triace 1 are 6 and 1 in the 7-edron, and 4 and 1 in the 5-edron. The nodal summits and faces are 3 and 1 in the 5-edron, and 4 and 1 in the 7-edron. No other mode of autopolar signature is possible in these.

Every odd-based pyramid is utrally autopolar. The 6-edral and 8-edral pyramids may receive either of the signatures following:—

1 2 3 4 5	1 2 3 4 5 6 7
5 4 3 2 1	7 6 5 4 3 2 1
1 2 3 4 5	1 2 3 4 5 6 7
4 5 1 2 3	5 6 7 1 2 3 4,

the first of which lines exhibits nodal faces and summits 31 and 41, while in the second every triangle is opposite its polar triace, and no face or summit is nodal.

No pyramid is enodally autopolar, *i. e.* capable of *only* enodal signature. If we draw a 7-gon whose summits are 1234567, and then the dotted lines 73 and 75, and next taking three points in it, complete the 5-gon 34089, and join 93, 92, 81, 87, 06, 05, 04, we can sign the faces thus:—

045=1, 506=2, 6087=3, 781=4, 1892=5, 293=6, 39804=7, 2371=0, 3754=8, 567=9. The type now represents an enodally autopolar 10-edron, in which no pair of gamics meet each other, or can by any autopolar arrangement be made to meet. The 18 edges of the solid are well represented thus, the odd places in a quadruplet showing summits, and the even, faces:—

1520	2630	3748	4158	5269	6379	7410	0783	8795
0251	0362	8473	8514	9625	9736	0147	3870	5978.

The gamic pairs stand together, and no quadruplet exhibits fewer than four numbers. A nodally autopolar must always be, and a utrally autopolar may always be so signed, that two pairs of gamics shall exhibit in each quadruplet a duad of the form *aa*. In the above type it is observable that every duad, as 15, occurs four times. The same thing is to be seen in every autopolar type of edges.

If we make use of the closed 10-gon 1239804567, as directed in a paper "On the Representation of Polyedra," in the 146th volume of the Transactions of the Royal Society, a paradigm of this 10-edron can be written out, exhibiting to the eye all the faces, summits, angles, and edges of the figure.

The problems following are next proposed and solved.

To find the number of autopolar (r+2)*-edra generable from the* (r+1)*-edral pyramid.*

The answer is, $(r>3)$,

$$\frac{1}{4}\left\{(r^2-3r)4_r+(r^2-3r+2)4_{r-2}+(r^2-2r-3).2_{r-1}\right\},$$

where the circulator $s_r=1$ or $=0$ as r is or is not $=sm$.

To determine the number of autopolar $(\mathrm{r}+3)$*-edra generable from the* $(r+1)$*-edral pyramid.*

The solution is, $(r>3)$,

$$\frac{1}{24}\left\{r^4-6r^3+11r^2-36r+24+9r^2.2_r+(r3+29r+60)2_{r-1}\right\}$$
$$-0^{(r-4)^2(r-7)^2}-2\cdot 0^{(r-5)^2}.$$

Hence it appears that there is one autopolar 6-edron, not a pyramid, and five autopolar 7-edra besides the 7-edral pyramid, viz. three generable from the 6-edral and two from the 5-edral pyramid.

The problem of enumeration of the x-edra may, by a slight extension of the meaning of *partition*, be stated thus: *to determine the* k*-partitions of a pyramid*; and this depends on the problem, *to find the* k*-partitions of a polygon,* and on this, which is nearly the same question, *to find the* k*-partitions of a pencil.*

By the k-partitions of a p-gon is meant the number of ways in which k lines can be drawn not one to cross another, and terminated either by the angles of the polygon, or by points assumed upon its sides or within its areas so as to break up the system of one face and p summits into a system of $1+h$ faces and $p+i$ summits, where $h+i=k$; it being understood that if a point be assumed within the area, three lines at least shall meet in it, and if on a side, one segment of it shall be counted among the k lines. The number of k-*partitions proper,* for which $i=0$, or of ways in which k-diagonals can be drawn none crossing another, is—

$$\frac{p^{k|1}.(p-3)^{k|-1}}{\lceil k+1\ .\lceil k+2},$$

which is also the number of ways in which a pencil of p rays can be broken up into $p+k$ pencils, by the addition of k lines, each one connecting two pencils.

COMMUNICATIONS RECEIVED SINCE THE END OF THE SESSION.

By a Resolution of Council of the 26th of June, 1856, the President and Officers are henceforth authorized, at their discretion, to print in the 'Proceedings' abstracts of Papers received during the Recess, without waiting until such Papers shall have been read to the Society.

I. "Chemical Examination of Burmese Naphtha, or Rangoon Tar." By WARREN DE LA RUE, Ph.D., F.R.S., and HUGO MÜLLER, Ph.D. Received August 1, 1856.

In several localities of the kingdom of Burmah, there emanates from the soil in considerable quantity a peculiar oleaginous substance, which is employed for a variety of purposes, but chiefly as a lamp-fuel and as an unguent, by the natives, and exported in moderate quantities under the name of Burmese naphtha, or Rangoon tar.

It is obtained by sinking wells of about 60 feet in depth, in which the liquid is collected by the miner as it oozes from the soil.

At the common temperature this substance has the consistence of goose-fat; it is lighter than water, has usually a greenish-brown colour, and possesses a slight odour, peculiar, but not disagreeable. It consists almost entirely of volatile constituents.

Burmese naphtha has already attracted the attention of other chemists; at present we refrain from entering into a discussion of their results, since it is our intention to give a full history of this remarkable natural product when, after the completion of our experiments, we shall have the honour of submitting to the Royal Society a detailed account of our investigation. The object of the present communication is to trace a mere outline of the results at which we have arrived up to this moment.

The circumstances under which petroleum—for this is the collective term which comprehends a great variety of oily emanations similar to Burmese naphtha—occurs in nature, all tend to prove that these substances are the products of a slow destructive distillation of the residuary matter of a primeval creation: this being admitted, the

idea naturally suggested itself of examining this substance in comparison with the products of artificial destructive distillation.

With this view, one of us* was induced to procure, through the intervention of a friend, several tons of Rangoon tar, which was carefully collected at the source, and transmitted to Europe in well-secured vessels. Our experience in the course of this inquiry, has shown that this quantity, large as it may appear, was by no means too ample a supply. Burmese naphtha contains indeed so great a variety of substances, and some of them in so exceedingly minute a proportion, that even the large amount of material at our disposal was insufficient for the complete examination of several constituents, the presence of which we had succeeded in establishing beyond a doubt. As an example, we may state that Burmese naphtha contains small quantities of organic bases, the study of which we were compelled to postpone to a later period, when an additional quantity of material, which is now on its way to Europe, will have come to hand.

We have already mentioned that Rangoon tar is almost entirely volatile, and preliminary experiments proved to us that the distillation could be effected most conveniently, and with less danger of obtaining products of decomposition, in a current of steam; first of a temperature of 100° C. (212° F.), and subsequently of steam superheated by passing, before it entered the still, through a system of pipes the temperature of which could be regulated. Treated in this way, it furnishes 96 per cent. of volatile products, fluid and solid.

Steam of 100° C. (212° F.) carries over 11 per cent. of a volatile oil perfectly free from solid hydrocarbons, which at that temperature are entirely retained in the distillatory apparatus. Between the temperatures of 110° and 145° C. (230°–293° F.), 10 per cent. of a further distillate is obtained, which is almost free from solid hydrocarbons. The temperature may be raised to 160° C. (320° F.) without materially augmenting this per-centage; but on gradually increasing the temperature of the steam to the fusion-point of lead, the operation yields 20 additional per cent. of distillate, which retains its fluidity at 0° C. (32° F.), notwithstanding the presence in it of an appreciable quantity of solid matter. At this stage of the process the products of distillation begin to solidify on cooling, and about 31 per cent. of a crystalline material is obtained sufficiently consistent

* Warren De la Rue.

to be submitted to pressure. After this the consistence of the products of distillation begins to diminish; and whilst the temperature of the steam is considerably raised, 21 per cent. of a mixture of solids and liquids distil, the latter predominating especially as the operation proceeds.

In the last stage of the process the distillate completely changes its character, becoming very dark in colour, of a pitch-like consistence, and exhibiting scarcely an indication of the presence of crystalline matter. When this product, which amounts to about 3 per cent., has passed over, there remains in the still a coke-like mass, which contains a small quantity of earthy impurities.

Although there is a considerable difference between the specific gravities of the first and last fractions of the distillates, all the products of distillation, like the original oil, are lighter than water, and could be separated therefore by means of the well-known apparatus (called a Florentine flask) employed in the distillation of essential oils.

By exposing the distillates obtained beyond the temperature of 145° C. (293° F.) to a freezing mixture, nearly all the crystalline matter solidified, and became removeable by means of filtration and pressure. It was thus ascertained that Rangoon tar contains from 10 to 11 per cent. of solid constituents (paraffin).

Solid Constituents.

The solid product, when removed from the fluid hydrocarbons, still retains a portion of the latter with much obstinacy; in order to purify the solid, it has to be subjected to the action of boiling concentrated sulphuric acid, and to be subsequently washed, first with an alkaline solution, then with water. On redistillation, the paraffin is obtained quite white, but even now it still retains some fluid hydrocarbons which have resisted the action of the sulphuric acid; the greater part can be removed by pressure between folds of cloth in a powerful hydraulic press and subsequent exposure for some months to the air, in which the fluids gradually disperse. By fractional crystallization from hot alcohol, we have been enabled to separate the solid product into at least two distinct compounds, which appear to have the same per-centage composition, agreeing either with $C_n H_n$ or $C_n H_{n+1}$, but which differ from each other in their physical properties. By the action first of sulphuric acid saturated with anhydrous acid,

then with *anhydrous* sulphuric acid itself, we believe that we shall obtain compounds which will enable us to determine a rational formula for each of these interesting bodies.

Liquid Constituents.

In order to purify the liquid constituents of Burmese naphtha, they were, after the separation of the solid portion from such as contained any, twice redistilled in a current of steam, first of 100° C. (212° F.), and subsequently of superheated steam, the temperature of which was gradually raised. In the redistillation, however, steam of only 100° C. (212° F.) was found to carry over fluids which boiled at a temperature as high as 300° C. (572° F.).

A further separation of the various products was effected by repeated fractional distillations; but no absolutely constant boiling-points could be obtained, notwithstanding the great number of distillations and the large quantity of material at command. It is true that considerable portions of distillates could be collected between certain ranges of temperature, tending to indicate a constant boiling-point; nevertheless it soon became evident that distillation alone could not effect the separation of the various constituents, and that recourse must be had to other processes. The employment of concentrated sulphuric acid first suggested itself, and by its means a whole group of hydrocarbons could be removed from the distillates, the residue consisting of hydrocarbons, on which it had no action. This was an important step; but recourse was subsequently had, with even more success, to the action of strong nitric or a mixture of nitric and sulphuric acids, by which means a series of nitro-compounds were obtained, which presented the advantage of being more easily studied than the sulpho-acids. The nitric-acid method, which has already been described at some length*, promises to be of general applicability in the separation of complex mixtures of hydrocarbons, and has, in the hands of Mr. C. Greville Williams†, been lately employed with advantage in the investigation of "Some of the products of the distillation of Boghead Coal at low temperatures."

* In the specification of a patent granted to Warren De La Rue, Dec. 23, 1854, and entitled "Improvements in treating products arising from the distillation of a certain Tar or Naphtha, to render the same suitable for dissolving or removing Fatty or Resinous Substances."

† Proceedings of the Royal Society, vol. viii. p. 119.

Hydrocarbons separable by Sulphuric Acid and Nitric Acid.

The proportion of hydrocarbons removeable from the various distillates by means of concentrated sulphuric acid, nitric acid, or a mixture of both acids, is in most cases small when compared with the part not acted upon; it increases generally, however, with the boiling-point of the fluid, varying from less than one-tenth to nearly one-third part of the original compound-hydrocarbon. The nitro-compounds obtained by means of strong nitric acid are fluids at the lower end of the series, whilst with hydrocarbons boiling above 200° C. (392° F.) they are of a resinous consistence; they frequently retain with obstinacy a portion of the not-acted-upon hydrocarbons, and more especially if the experiment be conducted upon a small scale; nitro-compounds are sometimes obtained which float upon water, in consequence of this retention of the lighter hydrocarbons.

In submitting the hydrocarbons to the action of the acids, we have invariably selected fluids which from their boiling-points appeared to be within certain limits homogeneous; but notwithstanding every possible care in this selection, we have always obtained more than one sulpho-acid and more than one nitro-compound, as the case might be, and we have experienced very considerable difficulties in the separation of the mixed products. In the case of nitro-compounds, advantage has sometimes been taken of their convertibility by a mixture of sulphuric and nitric acids into di- and tri-nitro-compounds, which admitted of fractional crystallization from various media. We have been thus enabled to isolate the following compounds, the analysis and properties of which place their existence beyond doubt, namely,—

Nitrobenzol,
Dinitrotoluol,
Trinitroxylol,
Sulphocumolate of barium;

and therefore it is evident that the Burmese naphtha products contain the corresponding hydrocarbons, namely,—

Benzol..........	$C_{12} H_6$
Toluol..........	$C_{14} H_8$
Xylol	$C_{16} H_{10}$
Cumol	$C_{18} H_{12}$

But we have found that the foregoing are by no means the only hydrocarbons separated by sulphuric acid and nitric acid, and we hope to establish the existence of other series containing terms isomeric with, but differing in properties from, benzol and its homologues; we have, moreover, good reason to suspect the presence of other compounds even less linked with the benzol series.

Action of Reducing Agents on the Nitro-compounds.

In order to throw further light on the constitution of the hydrocarbons in Burmese naphtha, removeable by the before-named acids, we have submitted the several nitro-compounds to the action of reducing agents. As was to be expected, our nitrobenzol yielded an abundant supply of aniline when distilled with acetic acid and iron turnings, thus confirming the existence of benzol beyond all possible doubt. In a similar manner the presence of toluol was further established by the preparation of nitrotoluol and toluidine. Béchamp's method was, however, not equally applicable in all cases, so that Zinin's original sulphide of ammonium process was resorted to; by its means we have obtained several new bases, and among them one crystallizing beautifully in long needles, having the appearance and colour of alizarine. Some time must however elapse before the great number of new bodies can be fully studied.

Hydrocarbons not acted upon by Sulphuric and Nitric Acids.

The hydrocarbons which resist the action of monohydrated sulphuric and nitric acids form, as has been before stated, by far the larger portion of the distillates obtained from Burmese naphtha. When purified by washing from adhering acid, by fractional distillation, and finally by rectification in a current of dry hydrogen gas over the liquid alloy of potassium and sodium, they are obtained almost inodorous and perfectly colourless. Thus purified, they are very fluid, and retain their fluidity even in the intense cold produced by a mixture of solid carbonic acid and ether. No absolute fixity of boiling-point could be obtained in any of the products; nevertheless a much greater constancy in this respect was observed than with the hydrocarbons before treatment with strong acids. The lowest boiling-point obtained was 50° C. (122° F.); the highest, being far beyond the range of the mercurial thermometer, was not ascertained. The

specific heat of the vapour of all this series of hydrocarbons was ascertained to be very small, a fact which we believe accounts in some measure for the difficulties we experienced in the fractional distillations. For the purpose of analysis, we have contented ourselves with selecting such products as boiled within the same 5° C. (9° F.) of the thermometric scale. All the analyses tend to prove that the ratio of carbon to hydrogen increases slowly with an increase in the boiling-point, and to negative the not improbable assumption of the carbon and hydrogen being combined in equal equivalents. The general formula $C_n H_{n+1}$ agrees best with our results, and indicates a probability of the Burmese naphtha containing several radicals or their hydrides.

Our endeavours to obtain definite substitution-compounds by means of bromine and chlorine have been attended with only partial success. Chlorine gas acts slowly in the dark, but more quickly with the aid of diffused daylight; pentachloride of antimony, on the other hand, acts with so much violence that explosions frequently ensue.

Bromine appears to separate the hydrocarbons into two distinct bodies, a circumstance which throws some doubt upon their simple constitution. Hydrated sulphuric acid saturated with the anhydrous acid likewise produces a separation of the hydrocarbons, absorbing one portion and leaving the other unacted upon; anhydrous sulphuric acid, on the other hand, in some cases completely absorbs the whole, sometimes with evolution of sulphurous acid. The copulated sulpho-acids which are produced in these cases will probably enable us to clear up the enigma of the composition of the hydrocarbons; we think it better therefore not to lay much stress upon the Radical or Hydride hypotheses until further experiment has thrown more light upon the subject. Nevertheless we may state, that by operating upon a fluid boiling between 90° and 100° C. (194°–212° F.) with chlorine for some months, we at last obtained a cessation of all action, and a chlorine compound resulted, which, when purified and analysed, gave numbers agreeing perfectly with $C_{26} H_{22} Cl_6$, corresponding to a hydrocarbon, $C_{26} H_{28}$, a formula with which the analysis of the original hydrocarbon was perfectly consistent, although its boiling-point pointed rather to a lower formula.

Action of Oxidizing Agents on the Hydrocarbons.

By the action of boiling diluted nitric acid, continued for many months, on the hydrocarbons not acted upon by concentrated sulphuric and nitric acids, oxidation gradually takes place, and a great variety of acids are produced, among which we have isolated succinic acid, and several others belonging to the series $C_n H_{n-2} O_8$.

Oxalic acid, the lowest term of this series, could not be traced; there occurs, however, in these products several of the volatile acids of the acetic acid series, but in very small quantities. The rough distillates obtained from the Burmese naphtha, when treated in the same manner, yield, in addition to the foregoing, several aromatic acids, derivatives of the benzol series and its isomers, differing however from any acids at present known. Other oxidizing agents have been employed by us, but not with such marked results.

November 20, 1856.

Dr. MILLER, V.P., in the Chair.

In accordance with the Statutes, notice was given of the ensuing Anniversary Meeting for the election of Council and Officers.

Edward William Binney, Esq., and Cæsar Henry Hawkins, Esq., were admitted into the Society.

Wilhelm Karl Haidinger and Antonio Secchi were balloted for and elected Foreign Members of the Society.

The following communications were read:—

I. "Experimental Researches on the Organ of Vision.—Part I. Microscopic Examination of the Circulation of the Blood in the Vessels of the Iris and of the Choroid Membrane, &c." By AUGUSTUS WALLER, M.D., F.R.S. Received July 10, 1856.

(Abstract.)

In a former paper on the section of the optic nerve, the author described a process of producing temporary extrusion of the eyeball from the socket in the living animal. Although adopted in the first instance merely for the purpose of dividing the optic nerve *de visu* with as little injury as possible, the same means of exposing the eyeball may be advantageously employed for studying various other points relating to the physiology of the eye. In the first place, as the eyeball is so much protruded from the orbit, Kepler's experiment on the eye removed from the body, showing that external objects form inverted images on the retina, may be performed on the living animal. For this purpose it is merely requisite to place a bright object obliquely before the pupil, a candle for instance, in order to ascertain that a reversed image of it is formed on the opposite side of the eye. On a young rabbit

placed in a dark room, particularly if an albino animal is used, the inverted image of the candle, although having to traverse the choroid, the sclerotic and the muscular parts, is perceived bright, and tolerably well defined. When this bright spot is examined with a lens or a compound microscope, it is found to be sufficiently illuminated to allow of the examination of the motion of the globules of the blood. On the young guinea-pig the same may be still more easily examined, but it is particularly the albino rat or surmulot (*Mus decumanus*) that the author has found most suitable for these observations, exophthalmosis being most easily produced on this animal, while the eye is so transparent and the iris so close to the cornea, that the circulation of the blood-globules in the vessels of the iris may be observed. At the same time, beneath the iris and sclerotic may be seen the ciliary processes from their origin at the ora serrata to their anterior extremity, where they are seen to form a circular crown with a serrated edge surrounding the crystalline lens. The vessels of the ciliary ligament of the choroid are likewise rendered accessible to actual inspection during life.

For the purpose of examination, the animal is secured by a few turns of a band, about $1\frac{1}{2}$ inch in width, passed round its body so as to confine the limbs. It is then placed on a narrow slip of cork, to which it is fixed by some turns of string. When steadily fixed, exophthalmosis is produced and maintained by passing a strong thread around the slip of cork and the head of the animal close to the eye, alternately in front and behind it. When the compound microscope is used, it will be found requisite to place the animal sidewise, so as to direct the object-glass over the bright image above mentioned, which is always formed when the eye is placed before any bright light.

The author first describes the vessels of the conjunctiva over the sclerotic and cornea. These vessels form a network behind and before the ciliary ligament, and join into numerous small trunks which unite with a circular ciliary vein running all round the ciliary ligament. In the corneal conjunctiva the vessels may be traced towards the summit of the cornea over the outer half of its surface, and even beyond. They are seen to commence internally, as if with free extremities, from whence the blood is seen circulating rapidly towards the circumference of the cornea. Then large meshes soon

form, which become smaller as they approach the ciliary ligament, where they terminate in a circular vein surrounding the cornea. The rapid circulation of the blood at the apparent extremities of the vessels over the inner parts of the cornea, indicates their further continuation inwards or below, although the author has not traced them further over the cornea.

The vessels of the anterior surface of the iris, which Dr. W. next examines, consist of arteries and veins. The former are derived almost entirely from the two long ciliary arteries which arrive near the outer and inner angles of the eye, their course being traced over the anterior half of the sclerotic until they reach the great circumference of the iris, where they each subdivide into two equal branches, one inferior, the other superior, which diverge at an obtuse angle, each of them running in an oblique direction towards the edge of the pupil until they attain the inner third of the iris, where most of the final subdivisions are bent outwards. These four oblique branches form a symmetrical figure of a lozenge-shape over the iris, each branch giving off internal and external twigs, the former ending in the small circumference, the latter in the large circumference of the iris. The course of the blood may be watched in these vessels from over the sclerotic to their termination in the iris, but in general the current is too rapid to allow of the detection of the direction of the separate particles of the blood. It is only when the circulation becomes languid that the separate globules can be seen distinctly running in a centrifugal (*i. e.* arterial) direction.

By compressing the eye slightly, the passage of the blood may be retarded, and by that means be easily followed; but in so doing an error may possibly be committed respecting the arterial nature of these vessels, as the course of the blood is then generally reversed in the arteries, and will be seen to take a centripetal direction, sometimes for upwards of a minute, according to the amount of pressure. But in a short time, after oscillating within the vessels, the blood again resumes its natural course, which may be sufficiently regulated to enable us to watch the passage of the globules in the oblique branches and in their internal and external subdivisions.

The veins of the iris form two layers. The superficial layer comprises all the larger veins—generally twenty-three or twenty-four in number,—which radiate in a regular manner from the pupil out-

wards towards the ciliary ligament. They arise at the pupillary edge, each by two or three fine twigs, which quickly meet in a common trunk, or sometimes run separately as far as the outer half of the iris, where they unite in a common trunk.

The deep layer consists almost entirely of a fine network belonging to the radiating muscular fibres, and presenting a close analogy with the fine vessels supplying striated muscular fibre; the vessels being very minute, and the meshes elongated in the direction of the fibres. Sometimes the vessels from this layer unite into a small ramuscule, which empties into a radiating vein; at others they unite in a common trunk, passing beneath the ciliary ligament into the choroid.

The movement of the blood in the veins is generally not too rapid to distinguish the direction of the current and the separate globules, which appear to be constantly springing from around the edge of the pupil and pouring outwards along the veins of the iris into the choroidal and ciliary vessels.

When the pupil is contracted, the radiating vessels are rectilinear; but when it dilates they become curved and bent into zigzag and spiral forms, which are more or less curved or obtuse in proportion to the degree of dilatation of the pupil. This change in the form of the vessels does not appear to produce any difference in the speed of the current of blood.

Around the ciliary ligament are two and often three circular vessels, receiving the blood from the conjunctiva of the cornea and sclerotic, partly from the iris, and probably from the ciliary processes. Two of them are venous, and empty themselves into four large veins, corresponding to the anterior ciliaries, which arise in a perpendicular direction, and after following a rectilinear course over the sclerotic, finally end in the ophthalmic vein. The third circular ciliary vessel is of an arterial nature, as shown by the greater thickness of its parietes and the rapidity of its current.

The current of blood in these vascular circles is a most interesting object from the variety of its course, as into each anterior ciliary vein the blood is seen pouring out from the circular vein in two opposite currents, to be united into one in the larger vessel. The author also describes the appearance presented by the blood poured into the circular veins by their afferent vessels.

II. "Researches on the Action of certain parts of the Solar Spectrum upon the Iris." By E. BROWN SÉQUARD, M.D. Communicated by Dr. SHARPEY, Sec. R.S. Received July 10, 1856.

In 1847 I discovered that light has the power of acting directly upon the iris so as to produce there a muscular contraction, manifesting itself by the constriction of the pupil. If an eye taken out from the orbit is alternately exposed to light and darkness, we find that the pupil becomes alternately constricted and dilated*.

It was interesting to know whether the stimulation of the muscular fibres of the iris is produced by the chemical power of light or not. I had already found, in 1847, that only the parts of light which seem to have but very little chemical action, have the power of exciting contractions in the iris. But my experiments having been made with light passing through coloured glasses, were not decisive. Lately I have performed many other experiments in making use of light decomposed by the prism. In one case, with the assistance of Messrs. Dubosc and Nachet, jun., I experimented with electric light, and in the other cases I made use of direct solar light.

In all these cases the same results have been obtained. I uniformly found that the yellow part of the spectrum acted as well as undecomposed light, and that the other parts of the spectrum had either no action at all, or only a very slight one. The parts of the green and orange adjoining the yellow had a decided but very slow action. The two extremities of the spectrum, and the dark places in their neighbourhood, not only had no constrictive action upon the pupil, but did not prevent it from dilating, and the dilatation seemed to take place as quickly as when the eye was put in complete darkness.

From these experiments it appears that the power possessed by light, of stimulating the circular fibres of the iris, belongs not to its chemical or to its calorific parts, but to its illuminating elements.

* Comptes Rendus de l'Acad. des Sciences, vol. xxv. pp. 482 & 508; and Comptes Rendus de la Société de Biologie, vol. i. p. 40.

It seems therefore that it is not by a chemical action, but by a peculiar dynamical influence that light produces contraction of the iris.

The power of the iris to contract when stimulated by light lasts extremely long, particularly in certain animals. In the eel I have found the muscular irritability of the iris, in one case, lasting *sixteen* days, during the last winter, in eyes taken out from the orbit. This is an interesting fact, not only on account of the long duration of vitality in the iris, but on account of the conclusion that we are entitled to draw from it, that muscular fibres may be stimulated without the intervention of nerves. In the iris of the eel the nerve-fibres are found very much altered a few days after the extirpation of the eye from the orbit, and they are almost destroyed twelve or fifteen days after this extirpation, *i. e.* at a time where muscular irritability is sometimes still existing.

III. A paper was in part read, entitled "Photo-chemical Researches.—Part I. On the Measurement of the Chemical Action of Light." By Professor BUNSEN of Heidelberg, and HENRY ENFIELD ROSCOE, B.A., Ph.D. Communicated by Professor STOKES, Sec. R.S. Received November 12, 1856.

November 27, 1856.

Sir BENJAMIN C. BRODIE, Bart., V.P., in the Chair.

Dr. Noad was admitted into the Society.

In accordance with the Statutes, notice was given of the ensuing Anniversary Meeting, and the following names of persons recommended for election as Council and Officers for the ensuing year, were announced from the Chair:—

President.—The Lord Wrottesley, M.A.

Treasurer.—Major-General Edward Sabine, R.A., D.C.L.

Secretaries.—{William Sharpey, M.D.
George Gabriel Stokes, Esq., M.A., D.C.L.}

Foreign Secretary.—William Hallows Miller, Esq., M.A.

Other Members of the Council.—James Moncrieff Arnott, Esq.; William Benjamin Carpenter, M.D.; Arthur Cayley, Esq.; The Very Rev. The Dean of Ely; William Fairbairn, Esq.; Arthur Farre, M.D.; William Robert Grove, Esq., M.A.; Joseph Dalton Hooker, M.D.; William Hopkins, Esq., M.A.; William Allen Miller, M.D.; Lyon Playfair, Esq., C.B., Ph.D.; Rev. Bartholomew Price, M.A.; Sir James Clark Ross, Capt. R.N., D.C.L.; Rear-Admiral W. H. Smyth, D.C.L.; John Stenhouse, LL.D.; John Tyndall, Esq., Ph.D.

Mr. Grove, Dr. Hooker, Mr. Hopkins, Sir James C. Ross, and Dr. Tyndall were elected Auditors of the Treasurer's Accounts on the part of the Society.

The reading of Professor Bunsen and Dr. Henry Roscoe's Paper "On the Measurement of the Chemical Action of Light," was resumed and concluded.

(Abstract.)

The only instrument which has been applied to the measurement of the chemical action of light was proposed in 1843 by Dr. Draper of New York. The sensitive substance employed by him was a mixture of chlorine and hydrogen, and by measuring the diminution ensuing on exposure to light, he experimentally determined some important relations of photo-chemical action. Draper's instrument is, however, not adapted for accurate measurements, owing, in the first place, to the fact that the gas is subject to varying pressure; and, in the second place, that the statical equilibrium, which must exist between the free and dissolved gases, in order that the free gas should consist of equal volumes of chlorine and hydrogen, was never approached.

In order to obtain more accurate results than was possible with Draper's tithonometer, we sought for means of preparing a gas containing equal volumes of chlorine and hydrogen; this means was found, notwithstanding Draper's contrary statement, in the electro-

lysis of strong aqueous hydrochloric acid. A series of volumetric analyses proved that the gas thus evolved consisted, as soon as the requisite saturation had been attained, of exactly equal volumes of its component parts, and did not contain the slightest trace of oxygen or oxides of chlorine. Another series of experiments with gas, similarly prepared, but allowed to stand before analysis for many hours in the dark in closed vessels, proved that, at the ordinary atmospheric temperature, the gases do not enter into combination when the light is excluded. Being thus enabled to prepare a substance which undergoes decomposition on exposure to light, but does not change on preservation in the dark, we proceeded to construct an apparatus by means of which the laws of the chemical action of light might be thoroughly investigated. After many fruitless attempts, we have succeeded in constructing an instrument, by which not only accurate comparative determinations can be made, but which has enabled us to reduce the chemical action of light to an absolute measure.

The most essential conditions fulfilled by our instrument are the following:—

1. A continuous evolution of a gas consisting of exactly equal volumes of chlorine and hydrogen free from all foreign impurities.

2. Constant pressure on the gas and liquids throughout the apparatus.

3. Absence of all caoutchouc or other organic matter which might alter the composition of the gas.

4. Exclusion of all variation in the composition of the gas in the apparatus from exposing the saturated liquids to the light.

5. Complete establishment of the statical equilibrium between the free and dissolved gases.

6. Elimination of the disturbing action of radiant heat.

The instrument in which these conditions are fulfilled is constructed entirely of glass, and consists essentially of four parts, viz.—1, a decomposing tube in which the gases are generated from carbon poles; 2, a washing tube containing water, furnished with an air-tight glass stopcock; 3, the vessel in which the gases are exposed to the action of the light attached to the other parts of the apparatus by air-tight ground-glass joints; and 4, a horizontal tube on which the diminution of volume in the insolation vessel is observed by means of a millimeter scale.

When the apparatus is freshly filled with the requisite quantity of water, the pure electrolytic gas is allowed to pass through, certain necessary precautions being used, until a constant source of light, such as a coal-gas flame burning under certain circumstances, produces in equal times always the same alteration of volume. This constant maximum action is generally not reached until from eight to ten litres of gas have passed through the instrument, and the saturation has continued for from three to six days. As soon, however, as this maximum is attained, the instrument is ready for use, and preserves this constant sensibility for many weeks, requiring only a short saturation each day in order to fit it for accurate photo-chemical measurements.

On exposing the gas to the light, the quantity of hydrochloric acid formed does not at once attain the maximum: a certain time often elapses before any alteration of volume is perceptible; a slight action is, however, soon observed, and this gradually increases until the permanent maximum is reached. This phenomenon, to which we have given the name of photo-chemical induction, is one of great interest and importance, and will be specially studied in our next section. As the maximum action is not attained for several minutes after the first exposure, the observations can only be made use of as soon as the action for several successive minutes has become constant. By a combination of several actual observations, the differences between the indications are found to be very slight.

Found.		Mean.		Diff.
13·23	....	13·36	....	+0·13
13·50	....	13·36	....	−0·14
13·35	....	13·36	....	+0·01

A special investigation was conducted for the purpose of determining the effect produced by the heat evolved from the slow combustion of the chlorine and hydrogen. Experiment and calculation gave the following as some of the more important results:—

1. That the heat evolved in the insolation vessel from the combustion of the gases exerts no perceptible influence on the indications of the instrument.

2. That the slight diminution in volume which occurs in the first few seconds after exclusion of light, is entirely owing to a decrease of temperature from a cessation of the combustion.

In order fully to test our apparatus, we have observed the action effected by a coal-gas flame of constant dimensions on our apparatus on different days. Determinations made in June last gave the following results :—

	Action in 1 min.		Diff. from mean.
11th June ..	14·00	....	+0·01
12th „ ..	14·26	....	+0·35
13th „ ..	13·80	....	−0·11
19th „ ..	13·83	....	−0·08
21st „ ..	13·88	....	−0·03
26th „ ..	13·72	....	−0·19
Mean value..	13·91		

Observations made with the constant flame placed at different known distances from the insolation vessel, proved that the amount of chemical action produced varied inversely as the square of the distance; and experiments made in September with the standard flame gave results which agreed most exactly with those obtained in June. From the exact agreement of these various observations we are assured of the accuracy and reliability of the measurements made with our instrument.

In order to see whether the variation of the atmospheric temperature exerted any influence on the sensibility of the electrolytic gas, we saturated the apparatus at various temperatures lying between 18° and 27° C., and found that the difference between the action at any two temperatures lying between the above degrees was so slight, that it did not exceed the unavoidable errors of experiment.

In the next communication we shall consider the relations of the interesting phenomena of photo-chemical induction.

December 1, 1856.

ANNIVERSARY MEETING.

The LORD WROTTESLEY, President, in the Chair.

Dr. Tyndall reported, on the part of the Auditors of the Treasurer's Accounts, that the total receipts during the last year, including £360 arising from the sale of a portion of the Acton Estate, and £200 bequeathed by the late Henry Lawson, Esq., F.R.S., amounted to £3780 3*s.* 1*d.*, and that the total payments in the same period, including £560 invested in the Funds, amounted to £3643 8*s.* 10*d.*, leaving a balance due by the Treasurer to the Society of £136 14*s.* 3*d.*

The thanks of the Society were voted to the Treasurer and Auditors.

List of Fellows deceased since the last Anniversary.

On the Home List.

Hon. George Charles Agar, M.A.
James, Earl of Bandon.
Rear-Admiral Frederick William Beechey.
Sir William Edward Rouse Boughton, Bart.
Very Rev. Wm. Buckland, D.D.
Wm. Frederick Chambers, M.D.
Sir Alexander Crichton, M.D.
Sir George Duckett, Bart., M.A.
Charles Elliott, Esq.
Right Hon. Henry Goulburn.
George James Guthrie, Esq.
Rev. John Philip Higman.
Admiral Philip Parker King.
John Fletcher Miller, Esq.
Henry Charles, Duke of Norfolk.
Sir Benjamin Outram, M.D.
William Hasledine Pepys, Esq.
John Urpath Rastrick, Esq.
John Reeves, Esq.
James Meadows Rendel, Esq.
Samuel Rogers, Esq.
Daniel Sharpe, Esq.
William Swainson, Esq.
Sam. E. Widdrington, Capt. R.E.

On the Foreign List.

Jacques Charles François Sturm.

Withdrawn from the Society.

Sir John Hall, Bart. | Edward Hawkins, Esq.

Fellows elected since the last Anniversary.

John Hutton Balfour, M.D.
Edward W. Binney, Esq.
Sir John Bowring.
Sir John Fox Burgoyne, Bart.
Philip Henry Gosse, Esq.
Robert Harkness, Esq.
Cæsar Henry Hawkins, Esq.
Manuel John Johnson, Esq.
John Carrick Moore, Esq.
Henry Minchin Noad, Esq.
Edmund Potter, Esq.
Rev. T. Romney Robinson, D.D.
Henry Hyde Salter, M.D.
Archibald Smith, Esq.
Capt. Thomas A. B. Spratt, R.N.

Foreign Members elected.

Karl Haidinger. | Antonio Secchi.

Readmitted.

Robert William Sievier, Esq.

The President then addressed the Society as follows:—

Gentlemen,

Since we last met to celebrate the Anniversary of this time-honoured Institution, events have taken place which it would be improper to pass over in silence. I allude, of course, to the occupation of Burlington House, which we owe to the liberality and due appreciation of Science of Her Majesty's Government. Your Council have already taken measures for the purpose of communicating to you the most important information respecting our proposed migration, which may be said to have already commenced, though some time must yet elapse before it can be finally completed.

As soon as Her Majesty's Government had been apprised that this Society had accepted their offer of apartments on the site in question, in conjunction with the Linnean and Chemical Societies, directions were given for commencing the necessary works, and I have the gratification of being able to announce to you, that considerable progress has already been made: the eastern wing is ready for the reception of the University, and the Great Hall is nearly completed.

The main building will be shortly delivered over to our custody; and I entertain confident hopes that the removal of the Society may take place early in the ensuing year, though the Great Hall in the west wing may not be in a fit state for painting. This, however, will not prevent its temporary occupation until such time as it may be necessary to vacate it for the purpose of its final completion.

During the recess and progress of the works, suggestions of certain alterations in the official plans have been made, some of which have been acceded to by the Government, and I have every reason to believe that no unnecessary delay will be allowed to take place in finishing all that yet remains to be done.

On such an important question as an entire change of abode, and the abandonment of a locality occupied for so many years, and, as was truly said by a distinguished Fellow of our Society, associated with many hallowed names and reminiscences, it is impossible to expect complete unanimity of opinion, and there may be some among us who still doubt the propriety of the step which has been taken; but they will, I am sure, give those who have approved of the change credit for having been actuated solely by a sincere wish to promote the interests of Science and of this Society. I still entertain the sentiments on this subject which I took the liberty of expressing on two occasions when I addressed you from this Chair; indeed I am, if possible, more than ever persuaded that great and lasting benefit will accrue to Science from our removal to a site more accessible to the great majority of our Members, and from the other advantages which must follow in its train. It has been suggested, that the two Societies about to be associated with us, should for the future hold their meetings contemporaneously with those of the Royal Society; so that on those days on which the Linnean and Chemical Societies meet, their Members may be enabled to join us in friendly converse after the business of the evening has been concluded. I hope I am not too sanguine in anticipating great advantages from these assemblies of earnest cultivators of science, devoted followers of one of the most deeply interesting and important of all human pursuits, in a building not only adorned with the portraits of some of the most distinguished men who ever shed the lustre of their genius on the country which gave them birth, but containing on its walls, in convenient juxtaposition, three scientific libraries, the accumulated treasures of ages of un-

wearied research. It cannot be but that on such a spot, and in such society, even the diligent cultivator of science will be stimulated to greater exertions.

Your Council have also adopted a measure which cannot fail to be productive of benefit to experimental philosophers. It is well known that on some occasions, when money has been voted by the Council, on the recommendation of the Government Grant Committee, for the purpose of aiding scientific researches, a part of such money has been expended on delicate apparatus, necessary for the purpose of performing the requisite experiments. It is proposed that all such instruments shall become vested in this Society for the benefit of the scientific public; and the Council hope to be enabled to set apart a room for their safe custody, and perhaps for the performance of experiments; thus in some measure reverting to the practice of ancient times; with this difference, that, whereas the apparatus of those days was necessarily primitive and rude, the Instrumental Museum about to be constituted will probably contain some of the choicest specimens of the workmanship of our most accomplished artisans.

I cannot take leave of this subject without tendering our sincere thanks to Her Majesty's Government for providing us with so convenient and central an abode, a measure which will redound no less to the honour of those who conferred, than of those who received the valuable boon.

I have before alluded to a Report of the Parliamentary Committee of the British Association, addressed to that Association at Glasgow in September 1855, in which were embodied opinions of certain eminent cultivators of Science on the question,—Whether any measures could be adopted by the Government or Parliament that would improve the position of Science or its cultivators in this country?

The discussion of this important question by a Committee of the British Association at Glasgow, was followed by a motion in the House of Commons, during the last Session, by Mr. Heywood, in which he proposed that the question should be referred to a Committee of the House of Commons. The proposal was not entertained by the House; but in the discussion which took place, Lord Palmerston is understood to have expressed himself in terms from which it might be inferred that he would be willing to take into favourable

consideration any proposal for the benefit of Science that should meet with the general approval of its most trustworthy representatives; and the matter was deferred till the ensuing Session of Parliament, with the view partly, it is believed, of permitting the question to be meanwhile maturely considered by scientific men. Under such circumstances, your Council conceived that the period was arrived in which the most ancient and venerable of all the Societies existing in this country for the cultivation of Science was called upon to take some steps for the purpose of eliciting the opinions of its most active Members on a question of such vital importance as that above referred to, and which seemed likely again to occupy the attention of the Legislature at no distant period; and accordingly, on the 11th of July last, they resolved,—

"That it was expedient that the subject should receive the attention of the Council at an early period of the next Session, and that, as a preliminary step, its consideration should be referred to the Government Grant Committee."

In pursuance of this resolution, the Government Grant Committee met, and appointed a Sub-committee, consisting of seven Members of their own Committee, together with your officers, to prepare a Report to them on the subject. That Sub-committee met on the 7th of October; and on this occasion they had before them the replies to two Circulars, requesting opinions on the above question, the one dated the 16th of July, and addressed by myself to the Members of the Government Grant Committee; the other dated the 20th of August, and addressed to the Members of the General Committee of the British Association by the Secretary of that Association, in pursuance of a resolution passed at Cheltenham on the 13th of August last.

The Sub-committee were therefore in a very favourable position for considering the various important matters involved in the question on which they were summoned to deliberate; and they devoted two successive days to the consideration of their Report under circumstances peculiarly well adapted to elicit, by prolonged discussion, apart from the formality of ordinary meetings, the views and sentiments of individual Members.

In my former Address I alluded to a proposal to constitute a new Board of Science, somewhat analogous in its functions to the late

Board of Longitude, but extending its operations to the whole domain of Science. If this question be decided in the affirmative, an ulterior one will arise, and that is,—Whether the Government Grant Committee, either organized as at present, or under a new constitution, could perform those functions and act as such a Board? The proper determination of this question involves many grave considerations, to some of which I had the honour of directing your attention in my former Address, and on which I need not again enlarge. It will be generally admitted, that the Committee has satisfactorily performed the limited duties now confided to it, of distributing the Parliamentary Grant in aid of scientific researches. It may be doubted, therefore, whether it would be prudent to interfere with their performance of *that* task for the future; and it may be advisable not to disturb in any way the present relations between this Society and the Government. It is a more difficult point, however, to determine whether it would be proper to invest your President and Council, or the Government Grant Committee appointed by them, either with or without changes in its organization, with the functions which it may be proposed to confide to the Board above mentioned. It will be admitted, that its constitution ought to be such as should inspire the cultivators of Science, and the public in general, with confidence in its administration. It ought also to be such as to ensure the confidence of Government and Parliament, so that measures recommended by it should meet with a favourable reception from them. These questions will, doubtless, receive all that mature consideration which their important bearing on the interests of Science entitles them to, and I trust that before our next Anniversary measures may be adopted with the view of carrying out some of the recommendations contained in the Report to which I have already adverted.

On a former occasion I adverted to the necessity which existed for conceding a certain amount of public support to educational establishments, for the use of those who have not the means or opportunity of studying at the Universities, where the elements of physical science might be taught on a general and systematic plan. If this were necessary at any former period, it is still more called for now, when a system of examinations, prior to the conferring of appointments, seems to be gradually taking root and likely to form an important part of our administrative organization. There seems no reason why

the examination test should be confined to those who are candidates for Government, or other public situations ; on the contrary, it seems exceedingly desirable that a plan of admitting to the privileges of an examination all applicants, who desire to be examined, should be adopted by the State. The effect of such a measure upon education generally, will undoubtedly be most beneficial. A certificate might be given to each person examined, of the extent of his acquirements. It would then be the part of employers to ascertain whether the holders of these certificates possessed in addition such qualifications as would fit them for the situations at their disposal. To such measures as those above and before recommended, together with the cooperation of the Department of Science and Art, and the unremitting exertions of the Committee of Privy Council for education, presided over by its Vice-President, or a Minister of Public Instruction, we must look for a gradual development of a more general taste for scientific studies, with its certain accompaniment, a proper appreciation of scientific acquirements and researches. Then, and then only, will Science be generally recognized by a commercial and manufacturing population, who owe everything to her applications, not only as "*the very living principle and soul of the industrial arts,*" but as one of the most truly noble of all intellectual pursuits.

It is not extraordinary that those who are disposed to form a low estimate of the value of scientific research, should also entertain doubts as to the propriety of hazarding human life in its behalf. In the late discussions on the expediency of undertaking another Polar Expedition, it seemed to be assumed by some, that the well-grounded anticipation of valuable contributions to physical and geographical science, would not alone be sufficient to justify the exposing of the lives of gallant men to peril, not even of those who were most willing and anxious to be so employed, emulous of such distinction, and regardless of the risk. However this may be, it is certain that Science has sustained and does still sustain injury from the fear of offending against this popular notion, that it is wrong to hazard human life for such an object. In the case of the Polar Expedition, the risk would be very small, inasmuch as the exploration, instead of being as formerly a tentative one, embracing many thousand miles of unknown coast, would be confined to a fixed and limited locality hitherto unexplored and possessed of great scientific interest. Inde-

pendently of additions to our geographical and physical knowledge, the possible recovery of the magnetical observations, and the journals of the Franklin Expedition is a consideration of great moment, since the former must have been made by officers well trained to the task, with excellent instruments verified before the sailing of the Expedition, and in localities possessing peculiar interest in reference to the theory of magnetism; and the latter would doubtless contain a narrative of the deepest interest, not only to the cultivators of science, but to the public generally, and especially the relatives of the gallant men who are supposed to have perished. The Expedition is, however, also advocated on the score of humanity, for experienced Arctic navigators still think it not impossible that some survivors of the crews may be living among the savage tribes, whose lot is cast among those inhospitable and barren regions. But admit that there is danger in these enterprises,—Is it inglorious to perish in promoting human progress? You will not suspect me, I am sure, of being indifferent to the fate of brave men; but in fact it is well nigh impossible to add to our stock of physical knowledge without some risk to life. The Astronomer in his observatory, exposed night after night to the open air at a freezing temperature; the Chemist in his laboratory, among explosive and poisonous substances; the Surgeon who handles the dissecting knife,—all equally with the adventurous traveller expose their lives to peril. We know what was the opinion of the great Athenian moralist and martyr on this question, from that fine passage in which the dangers of military and civil life are so beautifully contrasted:—"I should have acted strangely indeed," says he, "if, having stood firmly in the post assigned to me by my general at Amphipolis, Potidæa and Delium, and braved every danger, I had turned coward and feared to die, when my God ordered me to be a philosopher and instruct mankind." Whether men perish in peace or war, if they fall in advancing civilization or arresting the progress of barbarism, what matters it whether their bones rest in a soldier's grave, or lie scattered, "as when one heweth wood," on the ice-bound shores of the Polar Sea? All are entitled to the Spartan epitaph, "Go tell our countrymen we lie here, having obeyed their commands,"—for all have alike fulfilled their mission.

Even the great Poet of the benighted middle ages introduces an

adventurous navigator addressing his crew, about to leave the narrow seas and launch out on a great and unexplored ocean, in these animating lines:—

> "Fatti non foste a viver come bruti,
> Ma per seguir virtute e conoscenza."

The interesting expedition to the Peak of Teneriffe, to which I called your attention on a previous occasion, has now been brought to a conclusion, and we may congratulate Admiral Smyth on the return of his enterprising son in safety, after having performed important services to astronomical and physical science. Great credit is due to Mr. Piazzi Smyth for the perseverance and skill with which he surmounted difficulties. The allusion to this Expedition imposes on me the pleasing duty of commemorating the liberality of Mr. R. Stephenson, M.P., in placing his yacht and her crew at the unreserved disposal of Mr. Piazzi Smyth for some months; a proceeding which deserves the grateful acknowledgments of all true lovers of science.

I understand that the value of the meteorological observations made by Mr. Smyth has been much enhanced by the excellence of the instruments supplied to the Expedition by the Board of Trade, after having been tested at that valuable establishment, the Kew Observatory.

Having had occasion to allude to this Observatory, I am sure you will be glad to hear that your Council have lately had an opportunity of proving, that their desire to uphold useful scientific undertakings is not exhibited alone in the support of establishments organized by themselves, and yielding fruits of which they reap the fame, but that they are willing to extend a helping hand wherever real Science has taken root, and has need of a friend. It had become necessary to light the Kew Observatory with gas, in order to prosecute conveniently and successfully the important processes there carried on of testing meteorological instruments for the Government and foreign nations, and the photographic registration of physical phenomena. An application was made to Government to defray the cost, estimated at £250. A long correspondence ensued, which is no bad illustration of the defects of the existing relations between Science and the Executive authorities of the State. The application was declined. Immediately upon receiving notice of this disappoint-

ment, your Council voted £250 from the Wollaston Fund to the British Association to be employed in lighting their Observatory, and the works are now in progress.

The Astronomers continue to add yearly to the catalogue of the planetary bodies of our solar system, several small planets revolving between Mars and Jupiter. The rapid progress of these discoveries is well illustrated by mentioning, that in 1852 no less than eight were discovered, in 1854 six, and in the present year five have been already added to the list; again commencing with 1847, every year has been signalized by the finding of some of these planetoids. They now amount in number to forty-two; and astronomers have found it necessary to assign, by agreement among themselves, the labour of observing a certain portion of them to particular observatories, each astronomer taking charge of, and making himself accountable for, accurate observations of some five to eight members of this curious group. Thus has a division of labour in astronomical research been fairly organized; and I cannot but think that the principle might be carried very much further with great advantage to the progress of knowledge. To take an illustration from astronomy only, on casting your eyes over the list of Observatories, both public and private, in the Nautical Almanac, you would be surprised at their number; now that list contains only a portion of the *private* observatories of this country, and such establishments are very numerous in the United States also. Now let us reflect for a moment what would be the effect if to each of these numerous observers, who is qualified for the task and possesses the necessary means, were to be allotted some one peculiar object of astronomical research, and this were pursued till the special purpose in view should be fully attained. The amount of work performed would surely be augmented to a great extent. The same principle is applicable, though perhaps in a less degree, to other departments of science. But I entertain little hope of any such scheme as that, which has been sketched out, being carried to a prosperous issue, till another revolution has taken place in Science, which I cannot but look forward to as eminently calculated to advance its progress; I mean a greater amount of intercourse between the members of the various Scientific Societies of Europe and America.

The benefits which such a measure would confer are numerous,

but time will not permit me to do more than hint at some few of them.

In the first place, then, the requirements of Science often necessitate large outlays of money on objects, the importance of which, in the present state of knowledge, can hardly be sufficiently appreciated by a majority of the members of any community. A Government, however enlightened itself, however much alive to the value of the measure suggested, may hesitate in such a case to take upon itself the responsibility of recommending that the whole cost should be borne by the particular nation whose welfare is committed to its care; this might be a step too much in advance of public intelligence and therefore likely to be condemned. Now there would seem to be no reason why, in such an instance as this, the various civilized nations should not agree to bear the cost between them; and I am persuaded, that if greater intercommunication took place between foreign cultivators of science and ourselves, this is a result at which we should at last arrive. A better illustration cannot be given of the kind of scientific undertaking, which might be thus parcelled out among various countries, than the scheme which has been for some time under discussion for establishing a reflecting telescope of considerable power in some convenient locality in the Southern Hemisphere, for the purpose of observing the Southern Nebulæ. It is not difficult to demonstrate the importance of this object. The great command of light possessed by the magnificent telescope of our late distinguished President, Lord Rosse, has enabled him to detect certain configurations in the Nebulæ visible in this country, which had escaped the notice of prior observers; I allude to the discovery of the spiral form of several of these curious objects. Now this is a fact of peculiar interest, as bearing upon important questions of physical astronomy, the question, *e. g.* whether certain laws prevailing in our own system, and even in many stellar groups comparatively near to us, extend to the very remote regions of space tenanted by the Nebulæ. Many ages may indeed elapse before these questions can be solved, but it is a duty we owe to posterity to supply the data required for solving them; and it is necessary for that purpose that accurate drawings should be now made of the present appearances of these objects, to be compared with the observations of after times. Lord Rosse is at present engaged in making detailed observations

and drawings of the appearances presented by Nebulæ visible in our own latitudes; and it is most desirable that a telescope, not much, if any, inferior in power to his, should be set up somewhere in the Southern Hemisphere to perform for the Nebulæ there visible the like office. Now the cost of such an instrument will be very considerable, and the expense will not be confined to its construction merely, but a large permanent outlay will be necessary for the maintenance of observers, and occasional repolishing of the specula, repairs, and so forth. Here is an object in which all mankind are interested, but of which the importance is not likely to be so well appreciated by the majority of even educated men, as to make them very willing to spend large sums upon carrying it out; it is therefore one to which the funds of several communities might usefully contribute a small share. Happily there are precedents for the adoption of such a course, for our Government has in one instance at least within my knowledge, and perhaps in others, liberally aided with its resources the labours of foreign men of science. Surely it is a pleasing spectacle to view several nations combining their resources to advance highly intellectual inquiries, in the success of which the whole world is concerned; and though it may not at first sight appear that any utilitarian end is likely to be promoted by the discovery of laws which prevail in regions so far removed from ourselves, still the whole history of Science shows, that useful applications are continually arising in quarters where they are most unexpected; and he who is thus deterred from prosecuting scientific discovery, in any quarter whatever, has watched the progress of the inductive sciences and the gradual growth of art to very little purpose. No knowledge of the laws of the grander phenomena of nature can possibly be a matter of indifference to us, when every day's experience more and more establishes the fact of the close analogies which subsist between all her operations. Who can say, therefore, that the lapse of ages will not reveal the effects of laws in operation in these mighty systems (where we are enabled, as it were, to take a comprehensive survey of the field of action), the observation of which may throw important light upon obscure phenomena, the mysteries of which have hitherto baffled the researches of our most acute philosophers? and once admit that these revelations *may* be made, and it may be safely predicated that a long succession of applications to

purposes of utility of the utmost importance to the well-being of the human race may follow in their train. It must be admitted that a great demand is made upon our forbearance by those who would depreciate scientific researches by the constant repetition of this senseless cry of "Cui bono." Well might one of our most distinguished philosophers, the worthy son of a renowned father, affirm, that he could not listen to it without a sense of humiliation. It is indeed incredible that so many men should be found to indulge in it in the present age, after the many astounding proofs which the history of the progress of knowledge affords, that had such sentiments prevailed in bygone times, human progress would have been almost arrested, and a very large proportion of the comforts and luxuries which we now enjoy would have been lost to mankind.

I have thus instanced one useful end which may be attained by a more extended intercourse between scientific men in different countries; I will now mention another. In the progress of the late investigations into the means of improving the relations between Science and the Government, it has occurred to me that if some conference could take place between representatives of the leading Scientific Societies of Europe and America, important information might be collected as to the general bearings of this question, by comparing the mode of dealing with it in different countries possessing administrative systems variously organized. There are many questions again of great importance to the progress of Science, as *e. g.* a liberal extension of *our* system of book-postage, which might be much advanced by prevailing on influential societies in different countries to urge their favourable consideration on their respective Governments; and it is really necessary that our energies should be directed to promoting the general diffusion of the scientific publications of all countries by every means in our power, in order that the evil may be arrested from which Science has already suffered so much; I mean the devotion of time and of talents of the most transcendent order in one country to the performance of tasks which have already been satisfactorily completed in another. I cannot but think, therefore, that periodical meetings of deputies from the principal Scientific Societies at some central spot on the continent of Europe would have a tendency to promote these and many other useful objects, which, if I were not afraid of trespassing on your patience, I might perhaps point out;

and a small portion of the funds of every Society would be well employed in defraying the cost. I will say nothing of the collateral advantages which would flow from such reunions, in making scientific men of different nations known to one another; but of this be well assured, that nothing, however seemingly trivial, which promotes good fellowship between neighbouring states,—nothing, however transitory in its duration, which brings men of different nations together in friendly and social converse on subjects of a neutral character, and altogether alien from topics which powerfully excite human passions,—can fail to exercise a most salutary influence in preserving peaceful relations and promoting the prosperity of the whole human race.

The time has now arrived when many considerations might induce me to resign the trust you have done me the honour to confide to me into your hands, but many important matters which have been undertaken since this distinguished office was conferred on me still remain unsettled; and in particular the decision of that interesting question, to which I have directed your attention,—I mean the relations which ought to subsist between Government and Science,—still hangs on the balance. As it was principally with a view of assisting in improving, if possible, those relations that I consented to accept an honour to which I felt myself unequal, so I hope to be enabled to prove, before I resign it, that on one important subject at least I have not laboured in vain.

The Copley Medal has been awarded to Professor Henry Milne-Edwards, who ranks by common consent as the most eminent living representative of the French School of Natural History; being distinguished alike for his extensive knowledge of Comparative Anatomy and Physiology, as well as of Zoology, and for the amount and value of his original contributions to these sciences. His whole career evinces the truly philosophic spirit in which he has laboured; and it would be difficult to name any existing Naturalist who has prosecuted his researches with equal success over so very wide a range of investigation.

Although Professor Milne-Edwards has furnished many valuable additions to our knowledge of the Vertebrated classes, yet it has been to the Invertebrata that his chief attention has been given; and in each of the three Cuvierian sub-kingdoms—Articulata, Mol-

lusca, and Radiata—his researches have been so important and successful, that what he has accomplished for either alone would suffice to establish for him a high scientific reputation.

His earliest labours were chiefly directed to the class *Crustacea*; of which (after having published numerous Memoirs on its various subdivisions) he produced in 1837–1840 an elaborate Monograph,—Anatomical, Physiological, and Systematic,—which is universally regarded as of pre-eminent merit, not only for its richness of detail, but also for the value of the general doctrines relating to Homologies, Development, Geographical Distribution, and other points of the highest physiological interest, which are enunciated in its pages.

The *Annelida* have also occupied much of Professor Milne-Edwards's attention; his researches on their structure, and especially on their development, were among the first, and are still among the most important of those numerous contributions which have of late added so much to our knowledge of this class; and they have served as models for all who have followed in the same path of inquiry. Some of the most important of these researches were made on the coast of Sicily, whither Professor Milne-Edwards was sent by the French Government in charge of an Expedition for the study of Marine Zoology.

His researches on the Circulation of the Mollusca, undertaken to clear up the difficulties in the asserted *phlebenterism* of the Nudibranchiata, have introduced a new and satisfactory mode of regarding the circulation of the Invertebrata generally, which throws light upon many obscurities, and solves many perplexities. Again, his researches on the Compound Ascidians have led to an entirely fresh appreciation of some of the most important points in the history of that group, which had escaped the penetration of Savigny;—more especially by making it clear that *propagation by gemmation,* which had been previously supposed to be a Zoophytic character, is equally true of the lower Mollusca; thus was the way prepared for the reception of the Bryozoa into that sub-kingdom.

The labours of Professor Milne-Edwards upon Zoophytes have not been less important or less successful than in the departments already named. He was (with his *collaborateur* M. Audouin) the first to observe and to appreciate the essential distinctions between the so-called Polypes of the *Flustra* and its allies, and the true

Zoophytes; he has done more than any other Naturalist to determine the boundaries of the group of *Polyzoa* (Bryozoa), under which these organisms—detached by common consent from the true Zoophytes—are now ranked; and by showing their very close relationship to the Compound Ascidians, he has established their title to rank in the Molluscous series.

The *True Zoophytes* have also received much of his attention; and his Monographs of various recent types of these, with his great work on the Fossil Corals of Great Britain,—executed by him (with the assistance of his friend and pupil, M. Jules Haime *) for the Palæontographical Society,—testify to the ability and success with which he has studied them.

Even in this imperfect sketch, it would be wrong to pass by Professor Milne-Edwards's admirable Memoirs on various members of the class *Acalephæ*, which are unsurpassed for their accuracy of anatomical detail and their justness of physiological deduction.

Whilst pursuing these laborious trains of original research, Professor Milne-Edwards has also been extensively engaged in the honourable work of Public Instruction; and both by his lectures and his writings he has applied himself to the diffusion of a sound taste for science through the community. And in whatever path he has followed, he has been distinguished by the same single-minded love of truth, and disregard of all selfish and personal considerations, as have pre-eminently characterized his scientific labours.

Professor Milne-Edwards,

Accept this Medal, the highest reward in our power to bestow, in token of our just appreciation of the labours of a life devoted to Natural Science, and distinguished by original views.

The Rumford Medal has been awarded to M. Pasteur for his discovery of the nature of racemic acid, and its relations to polarized light.

Chemists had long been acquainted with a peculiar acid, racemic, or paratartaric acid, which had the same composition as tartaric acid,

* M. Haime's recent death will be deeply lamented by all who were acquainted with the high promise of future distinction which he had already given.

and the same saturating power, and resembled it in its properties in a very remarkable manner. Yet the two acids were not identical, and the cause of their difference, notwithstanding their close agreement, remained a mystery. The resemblance between the two acids had been rendered still more striking on a comparison of the physical characters of their salts; for their crystalline forms were the same, their specific gravities the same, their double refraction the same. Yet the solutions of the tartrates rotated the plane of polarization of polarized light, while those of the racemates were inactive.

In a careful scrutiny of the crystalline forms of the tartrates, M. Pasteur was led to recognize the almost universal presence of hemihedral faces, of such a character that the two hemihedral forms which together make up the holohedral, were "dissymmetric," that is, could not be superposed on each other, but each could be superposed on the image of the other in a mirror. Sometimes the hemihedrism was indicated merely by the greater development of one pair of faces than of another pair. A hemihedrism of such a character that the two hemihedral forms were distinguished by right-handedness and left-handedness, seemed to be associated with the rotatory power of the solutions of the tartrates. If so, the crystals of the racemates might be expected not to exhibit the character of right- or left-handedness, since their solutions were known to be inactive on polarized light. Accordingly, on forming several of the racemates, and carefully examining the crystals, M. Pasteur found that the hemihedrism which had been observed in the tartrates was wanting in the racemates.

These patient and laborious researches in pursuit of truth were presently rewarded with an unexpected and brilliant discovery. On examining the crystals obtained in an attempt to form the double racemate of soda and ammonia, M. Pasteur observed that the crystals were hemihedral, and of two kinds, which differed only as to right-handedness and left-handedness; the one kind, which for distinction's sake may be called right-handed, absolutely agreeing with the corresponding double tartrate, the other with the image of the tartrate in a mirror. On separating the crystals of the two kinds mechanically, and dissolving them apart, the solution of the right-handed crystals was found to rotate the plane of polarization of polarized light right-handedly, like a solution of the tartrate, that of

the left-handed crystals left-handedly. These solutions yielded on evaporation, the one only right-handed, the other only left-handed crystals. The crystals of the two salts were purified by recrystallization, their acids isolated, and the chemical, optical, crystallographic, and pyroelectric properties of the acids themselves or their salts or solutions carefully compared. A like comparison was instituted between these acids and the well-known tartaric acid. The acid obtained from the right-handed crystals proved to be absolutely identical with tartaric acid in all its properties, that obtained from the left-handed crystals proved to be identical, so to speak, with the image of tartaric acid in a mirror, the two acids absolutely agreeing in all their properties except as to right-handedness and left-handedness. Where the one acid yielded crystals hemihedral right-handedly, the other yielded crystals exactly similar, except that they were hemihedral left-handedly; where the one yielded a solution rotating the plane of polarization right-handedly, the other yielded a solution rotating it left-handedly to the very same amount, and with the very same peculiar dispersion of the colours. On mixing equal quantities of the acids from the right-handed and left-handed crystals, racemic acid was reproduced.

Stimulated by this remarkable discovery, M. Pasteur has continued his labours in the same direction, and the results which he has since obtained are given in a series of papers published in the 'Annales de Chimie,' and extending nearly to the present time.

Hitherto no "active" substance (*i. e.* one whose solution has the power of rotating the plane of polarization of polarized light) has been obtained artificially from inactive substances, except in the case of the splitting up, or at least separation, of racemic acid into a right-handed and a left-handed substance; and this law seems worthy of the attention of chemists who attempt the artificial formation of the organic alkaloids. This law would have been violated had two acids which chemists had obtained from fumaric acid, an inactive substance, and which appeared to be identical with aspartic and malic acids respectively, been really so. But M. Pasteur found that these acids were inactive, unlike aspartic and malic acids, from which they also differed in some other respects.

The two acids obtained from racemic acid were found to be identical in their properties (except as to right-handedness and left-

handedness) *so long as they were mixed or combined with inactive substances only*, but M. Pasteur found that this is no longer the case when they are combined with active substances, as for example the organic alkaloids, in which case the salts obtained differ widely in solubility, crystalline form, &c.

It is to the stimulus afforded by the investigations of M. Pasteur that we must ascribe the more recent discovery by M. Marbach, that several crystals belonging to the cubical system possess the power of rotating the plane of polarization. Thus M. Pasteur's original discovery has already begun to bear fruit in discoveries made by others.

Dr. Sharpey, in the absence of the Foreign Secretary,

I request that you will transmit this Medal to M. Pasteur, in testimony of the value which we attach to his brilliant discovery.

Your Council have awarded one of the Royal Medals to Sir John Richardson. His claims to that honour as a most distinguished naturalist and scientific traveller, will I am sure be generally admitted. Sir J. Richardson's earliest work on Zoology appeared about the year 1823, but his first great work was published in 1829, namely the 'Fauna Boreali-Americana,' in which he has described the Quadrupeds and Fishes of the Arctic Regions, and with Mr. Swainson's aid, the Birds; the merits of this work, in the very accurate descriptions of the species, in the great amount of information on their habits and ranges, are admitted to be of the highest order. Since that period Sir J. Richardson has published largely on various branches of zoology, physical geography, and meteorology. His Reports to the British Association, on the Fishes of New Zealand and of China, are extremely interesting under many points of view. Another Report to the same body on the General Zoology of North America, is a most valuable contribution to science. His later works, which here must be more particularly considered, are the 'Zoology of the Voyages of the Terror and of the Herald,' in which he has described the Fishes and Reptiles collected during those expeditions, and given an account of some of the great extinct mammifers of the Arctic countries, with very interesting observations on their ancient relations and ranges. He has also lately contributed to the Geolo-

gical Journal a valuable paper, in which he has made known the presence of tertiary strata abounding with vegetable remains, in districts now rendered sterile by the extreme cold. Altogether I think there can be no doubt that the merits of Sir John Richardson, as a philosophical naturalist, are of a very high order.

It is not within our province to reward his other claims to distinction; but all will rejoice, that in the conscientious discharge of a delicate and important duty, the Council have been able to bestow a Medal on one, who has earned the applause of all who have watched his career, for his patient endurance and fortitude under incredible hardships in his first Arctic Expedition in company with Franklin, and again for his chivalrous self-devotion in the cause of friendship and science combined, at a period of life when most men resolve to rest from their labours, or at least would hesitate to encounter the fatigues and dangers of a Polar Expedition, the anticipation of which must have been more appalling to one, who had bitter experience of their painful reality.

Sir J. Richardson,

Accept this Medal as a token of our respect for your scientific labours and character.

The other Royal Medal has been awarded to Professor Thomson, whose labours in the cause of science are well known to scientific men. Yet the brief reference which can now be made to the Memoirs which he has written, will convey but an imperfect notion of the services which he has rendered; for the zeal with which he is inspired, his clear apprehension of mathematical and physical truths, and the freedom with which he communicates his ideas, have powerfully contributed to stimulate others in the pursuit of truth, and direct them into right paths. Shortly after graduating in the University of Cambridge he undertook the task of editing the Cambridge Mathematical Journal, which under his auspices was placed on an enlarged basis, under the title of the 'Cambridge and Dublin Mathematical Journal,' and is well known to the mathematicians of Europe. This Journal, as well as its predecessor the 'Cambridge Mathematical Journal,' is enriched by numerous contributions from the pen of Professor Thomson on various subjects, especially the mathematical theories

of heat, electricity, and magnetism. Among these may be mentioned a masterly article in which he has shown the compatibility of the ordinary mathematical theory of statical electricity with various phenomena which had been supposed by some to militate against it; his deduction from mathematical principles of Faraday's law relating to the motion of a small paramagnetic or diamagnetic body in a magnetic field; and his method of electrical images, first communicated to the public at the meeting of the British Association at Oxford in 1847, by which he is enabled in an extremely simple and elegant manner to solve a variety of important problems relating to the distribution of electricity on conductors.

Called to the Chair of Natural Philosophy in the University of Glasgow in the year 1846, he has ever since continued to devote himself to science in the intervals of his necessary occupations, and has worked especially at his favourite subjects of heat and electricity.

Carnot long since developed the mathematical theory of the motive power of heat in a clear and satisfactory manner, assuming as an axiom the indestructibility of heat. But the important researches of Mr. Joule have shown that this axiom must be abandoned, for that heat and work are mutually convertible. The establishment of this point necessitated a reconstruction of the mathematical theory of the motive power of heat, a theory of much practical importance from its direct bearing on the steam-engine, and this task Professor Thomson accomplished in a series of papers published in the 'Edinburgh Transactions.'

Professor Thomson and Mr. Joule have for a long time been working together, and they are now engaged in a series of experimental researches on the thermal effects of fluids in motion. The expenses attending the prosecution of these researches have been defrayed by donations from the Government Grant, and the results already obtained, drawn up partly in the form of short provisional accounts, have appeared in the 'Philosophical Transactions' and in the 'Proceedings of the Royal Society.'

In connexion with this subject may be mentioned Professor Thomson's remarkable speculation as to the cause of the light and heat of the sun, which he refers to the impact of meteoric bodies circulating around that luminary and continually falling in. The

opinion of scientific men seems to be divided as to the reception of this theory; but whatever may be thought of its truth, it has at least the merit of referring the light and heat to known causes.

The mathematical theory of magnetism was developed by the illustrious Poisson, but was made to rest on foundations in some respects too speculative. This subject has been taken up by Professor Thomson, who in a lucid and satisfactory manner has placed the theory on the basis of observed facts, so as to render it independent of any ulterior suppositions which may be adopted respecting the nature of magnetism. Two papers on this subject are published in the 'Philosophical Transactions,' and others, containing the theory of magnetic induction, are promised. More recently Professor Thomson has published a series of papers devoted to the mathematical theory of the submarine telegraph, and has been engaged in a series of experimental researches relating to voltaic electricity, which formed the subject of the Bakerian Lecture delivered in the session just concluding, and of which the detailed account will shortly be in the hands of the Fellows of the Society.

PROFESSOR THOMSON,

Accept this Medal in testimony of our admiration of your able mathematical and physical researches.

Obituary Notices of deceased Fellows.

JAQUES CHARLES FRANÇOIS STURM was born at Geneva in September 1803, of a family which had quitted Strasbourg in the middle of the last century, where one of his ancestors had been President of the Republic at the period of its contests with the Emperor Charles V., and another had attained a distinguished reputation for his writings on jurisprudence and theology. After completing his school education and his classical studies at the College with remarkable success, he became in his fifteenth year a student of the University of his native city, where his rapid progress in the study of mathematics and philosophy attracted the marked attention of the well-known geometer Simon Lhuillier, who fully anticipated the eminence which he was afterwards destined to attain.

The sudden death of his father, leaving his mother and four children, of whom Charles was the eldest, without any adequate maintenance, compelled him, before the close of his seventeenth year, to resort to private tuition for the support of himself and his family; and three years afterwards he was recommended to the Duc de Broglie, as tutor to the brother of Madame de Broglie, the son of Madame de Stael. At the close of the year 1823, he accompanied his pupil to Paris, and though he shortly afterwards returned to Geneva, he found no sufficient occupation there, and he finally resolved, in company with his intimate friend and school-fellow, M. Colladon, the present distinguished Professor of Physics at Geneva, to seek his fortune in the great city, which was then, and had long been, the undisputed metropolis of European science. Sturm had already become very favourably known to mathematicians by several articles in the 'Annales de Mathématiques' of M. Gergonne, on different branches of analysis and geometry, and the strong recommendations which he and his companion bore with them from Lhuillier, and the kind offices of M. Gerono, made them known to Ampère, Fourier, Arago, and other eminent members of the Institute, who recommended them to pupils as a means of support. Sturm afterwards obtained employment upon the 'Bulletin Universel,' under Baron Férussac, and was, in fact, a subordinate in the office of that journal when he published his well-known Theorem. He and his friend speedily began to feel the influence of breathing in an atmosphere of science, and their joint labours were rewarded by a distinction of no ordinary importance, when the Academy of Sciences awarded to them the great prize of mathematics proposed for the best Essay on the Compression of Liquids.

The determination of the number of real roots of a numerical equation which are included between given limits, is a problem which had occupied the attention of the greatest analysts of the past age, of Waring, of La Grange, and more especially of Fourier, who of all other analysts had made the nearest approaches to its practical, though he had failed in its theoretical, solution: the attention of Sturm had been for some time directed to this class of researches, which he pursued with remarkable continuity and diligence, encouraged, as he himself assures us, by the instructions and advice of this eminent master. The result was the discovery of the theorem which

will be for ever associated with his name, and which conquered the difficulty which had embarrassed all his predecessors, and thus permanently extended the dominion of analysis; a rare good fortune, which though frequently denied to the most illustrious cultivators of the sciences, is always reserved to those only who are enabled, by the extent and accuracy of their knowledge and the clearness of their views, to follow out the glimmerings of light which escape the observation of ordinary eyes.

The memoir which contained this important theorem was presented to the Academy on the 25th of May, 1829, and rapidly conducted its author to fortune and public honours. The connexion of its author with the 'Bulletin Universel' enabled him to give an immediate account of his method to the world; the paper itself was not published till some years afterwards, in the "Mémoires des Savans Etrangers."

In the course of a few years he was chosen a member of the principal scientific societies of Europe: the Copley Medal was given to him by this Society: he was elected a member of the Academy, as the successor of Ampère, in 1836: in the same year he was made Professor of Mathematics, upon the special recommendation of Arago, at the Collège Rollin, Répétiteur at the Ecole Polytechnique in 1838, and in 1840 he was deemed worthy to succeed to the chair of Mechanics at the same school, which had been so long honoured by the occupation of Poisson, the most illustrious of the successors of La Place. It was not without some difficulty that the substantial rewards of his scientific achievement were obtained: he was a foreigner, and naturally placed at a disadvantage in a contest with native competitors. It is right to notice this, both for the honour of France and as a proof of the very high reputation which Sturm had attained.

The subsequent memoirs of Sturm, whether first presented to the Academy or not, were chiefly printed in the Journal of M. Liouville, an analyst of congenial tastes and pursuits with his own, with whom he lived on terms of the most affectionate friendship. Two of these memoirs, relating to the discussion of differential and partial differential equations, such as present themselves so commonly in the solution of the more important problems of mathematical physics, possessed a merit so extraordinary, that M. Liouville—a most compe-

tent judge, declared—at a time when he was himself a competitor with Sturm for a place in the Academy,—"that impartial posterity would place them by the side of the finest memoirs of La Grange." The first of these two memoirs was presented in 1833 to the concours for the great prize of Mathematics, to be awarded by the Academy in 1834 for the most important discovery in that science made known within the preceding three years. The Academy conferred the prize on Sturm—not for the memoir which he had submitted to the judgment of the Commission, but for that which contained his celebrated theorem and which had been presented in 1829. Other memoirs relate to optics, mechanics, pure analysis, and analytical geometry, and embrace the most difficult questions which have been treated in those several branches of science. One of the latest of these was a communication to the Academy on the theory of vision, and is remarkable both for the geometrical and analytical elegance with which many questions subsidiary to the theory are treated in it. It confirms generally,—with one important exception relating to the asserted muscularity of the crystalline lens and the changes attributed to its action,—the views of the late Dr. Thomas Young in his well-known memoir on this subject.

Sturm visited England in 1841, and gave the mathematicians with whom he conversed a high impression, as well of the extent of his knowledge as of his inventive power.

The health of M. Sturm, which had previously been remarkably vigorous, began to decline in 1851, probably in consequence of his laborious public employments and the unremitting severity of his studies: he died on the 18th of December last, to the deep regret of a large circle of friends and pupils, to whom he appears to have been singularly endeared by the modesty, truthfulness, and simplicity of his character. "To my eyes," said M. Liouville, in the discourse which he pronounced at his grave, "Sturm was a second Ampère: candid like him, and like him equally indifferent to fortune and the vanities of life: they both of them joined to great inventive powers, an encyclopædic range of knowledge: neglected and even despised by men of the world and the worshipers of station and power, but exercising an unmistakeable impression upon the youth of our schools, where genius never fails to produce its impression: possess-

ing, in fact, without appearing either to desire it or to know it, an immense popularity."

The Rev. William Buckland, D.D., F.R.S., F.G.S. &c., Dean of Westminster and Reader in Mineralogy and Geology in the University of Oxford, was born in the year 1784, at Axminster in Devonshire. In 1797 he was at Tiverton School; in 1798 he entered St. Mary's College, Winchester, and passed from it in 1801, to a scholarship in Corpus Christi College, Oxford.

Admitted Fellow of that College in 1808, he manifested a decided taste for the study of geology, then beginning to be heard of in Oxford in the lectures of Dr. Kidd, the respected Professor of Mineralogy, and beginning to be cultivated in London by the founders of the Geological Society. While yet a child, his attention had been caught by the 'Cornua Ammonis,' found in the rocks round his home; at Winchester he began to collect the sponges and other fossils of the Chalk; at Oxford he gathered the shells of the Oolite, and discussed points of natural history on the ascent of Shotover Hill with his frequent companion Mr. Broderip of Oriel College, who had himself drawn no small amount of knowledge of these subjects from the Rev. J. Townsend, the friend and fellow-labourer of William Smith. The fruits of his first walk with Mr. Broderip formed the nucleus of that large collection which forty years later he placed in the Oxford Museum.

In the period from 1808 to 1812, Mr. Buckland was frequently seen traversing on horseback a large part of the south-western districts of England, and collecting from these tracts, which had been the scene of Mr. Smith's earlier labours, sections of the strata and groups of their organic contents.

In 1810 and 1811 he visited with the same purpose the north of England, Scotland, Ireland, and Wales.

In 1813 he received the Professorship of Mineralogy in consequence of the resignation of Dr. Kidd; he became a Fellow of the Geological Society, and took his place among the most active and most eminent of the inquirers into the physical history of the earth. The lectures which he now delivered were not confined to mineralogy, but embraced the discoveries and doctrines of geology, and they

attracted in a high degree the attention and admiration of the University. At length, in 1818, geology was publicly recognized in Oxford by the establishment of a Readership for this branch of science, and Buckland was appointed to the office. From this period the Reader gave annually one course of lectures on mineralogy and one on geology, sparing no pains and no expense in preparing these instructive and suggestive discourses, in which the very latest discoveries always found place.

Among his early contemporaries in Oxford none were so conspicuous in the cultivation of geology as the Rev. J. J. Conybeare and the Rev. W. D. Conybeare, both of Christ Church; and it is gratifying to remember that the strictest personal friendship united these eminent men in their subsequent brilliant career. It was in concert with W. Conybeare that Buckland gave to the press his first important paper "On the Coasts of the North of Ireland*,"—the result of a vacation tour from Oxford in 1813; and Mr. J. Conybeare was his companion in a visit to Devon and Cornwall.

In his journeys to the south-west of England he frequently called on the Rev. Benjamin Richardson of Farleigh Castle, near Bradford, and the Rev. Joseph Townsend of Pewsey, ancient friends of William Smith, and themselves among the ablest cultivators of the new views in geology. The latter of these eminent men imparted to the Oxford Professor his first knowledge of the details of superposition of the Oolite and Greensand formations between Bath and Warminster.

In the year 1818 he was elected a Fellow of the Royal Society, and speedily justified his claim to this honour by communicating to the 'Transactions' his well-known account of the teeth and bones of the Elephant, Rhinoceros, Hippopotamus, Hyæna, &c., discovered in Kirkdale Cave, 1821 †. This Essay was honoured by the Copley Medal, and being soon after reprinted under the title of 'Reliquiæ Diluvianæ ‡,' became a powerful stimulus to the cultivation of Geology and Palæontology throughout the world. Before the issue of this remarkable work, the author had traversed France, Italy, the Tyrol, Holland, Germany, and Bohemia, bringing to the now celebrated Oxford Museum large and valuable collections, and to the geologists of England observations of phenomena then little known to them. One result of these successful labours was the election of Buckland to

* Trans. Geol. Soc. vol. iii. † Phil. Trans. 1822. ‡ 4to, 1823.

the Chair of the Geological Society in 1824; and while he held this office, he received, in 1825, a valuable acknowledgement of his established merit in the gift of a Canonry of Christ Church. A more important event followed,—the happy marriage of Mr. Buckland to the excellent lady whose diligent hands and devoted affection shared every toil and lightened every anxiety of his life.

In the same year appeared, from the united hands of Buckland and Conybeare, the "Survey of the South-western Coal district of England," which even at this day may be consulted as one of the best guides to the geology of the singular country which it describes*.

In 1826 and 1827, he revisited the Continent to explore parts of France, Germany, Austria, and Switzerland. He then recognized the comparatively late geological date of the great upward movement of the Alps†, and declared some of the highly inclined rocks to be contemporaries of our Lias and Oolite. The Bone Caverns of Lunel and the Grotto d'Oselles‡ then yielded to his strong arms and capacious bags many valuable spoils, now preserved in the Oxford Museum. In the five years ending with 1830, we find him presenting to the Geological Society ten memoirs relating to Continental geology, and special researches among the fossils of Portland, Lyme Regis and the Mendips, the Isle of Wight, the Isle of Purbeck, and the coast of Weymouth.

In the latter memoir he was associated with one of his most valued friends, Sir H. T. De la Beche. To this period belong many of those curious researches on Coprolites and fossil Sepiæ, which attest at once the sagacity and industry of the great explorer of ossiferous caves. In 1832 Dr. Buckland cooperated with Dr. Daubeny and some other of his friends in the preparations for the Meeting of the British Association in Oxford, and was elected President of that brilliant and important meeting. In 1836 appeared the Bridgewater Treatise, 'Geology and Mineralogy, considered with reference to Natural Theology,' 2 vols. 8vo; a work equally attractive and valuable to the student.

The volumes of the Geological Society subsequent to 1833 contain many valuable notices of the unwearied labours of Dr. Buckland, one of the later and more interesting being a paper on the "Glacio-Dilu-

* Geol. Trans. 2nd ser. vol. i.

† Ann. Phil. new series, i. 4, 450.

‡ Geol. Soc. Proc. vol. i.

vial Phenomena in North Wales*." He was a contributor to the Linnean Society of a paper on the adaptation of Sloths to their way of life† (1835); and furnished many essays and notices on special subjects of interest to the Philosophical Magazine, Silliman's Journal, the Edinburgh Philosophical Journal, and the Reports of the British Association. The list published by Agassiz of the works and essays which bear the name of Buckland, extends to 66—spread over the whole period of his life in Oxford since 1813. In 1845 he became Dean of Westminster, and changed his residence, but not his habits of mental and bodily exertion. Sanitary measures, amendments in his Cathedral, agricultural improvements, the potato disease,—all occupied his attention, and consumed his time, so that from this time he almost ceased to labour as an author, though he still continued with unabated zeal the duties of his Professorship.

Dr. Buckland's numerous publications include very largely the results of personal observation, on features of physical geography, the succession of strata, the distribution of glacial detritus, the structure, habits of life, manner of death, and mode of occurrence of extinct animals. To him, more than to any geologist, are we indebted for unexpected suggestions, curious inquiries, and novel kinds of evidence. Thus in Kirkdale Cave, the peculiar condition of the broken bones—the smoothed surfaces of some—the worn aspect of others—the condition of the teeth—the layers of Stalagmite—the 'Album Græcum'—became in the mind of Buckland evidence of the mode of life and death of the former inhabitants. The footprints of Cheirotherium were joined with the ripple-mark of the rain-spot to determine the character of the mesozoic shore:—Coprolites were searched for the food of the Ichthyosaurus; snails were studied to explain holes in limestone; gelatine was extracted from the Mammoth's bones; toads were enclosed in cavities to determine their tenacity of life; the living hyæna was set to crush the bones of an ox, and thus to furnish evidence for the conviction of the old midnight robber of preglacial caverns.

Of general views on geology, Dr. Buckland was sparing as an author, though frequently and eloquently he declared them as a Professor. Physical Geology in its higher forms had scarcely existence in the earlier part of his career. Instead of contributing to its

* Geol. Soc. Proc. vol. iii.

† Linn. Trans. vol. xvii.

progress in after-years, he laboured wisely and well in the rich field of special discovery: now collecting and describing the mighty reptiles like Plesiosaurus and Iguanodon, or the flying wonder the Pterodactylus; at another time studying the beaks of Chimæra, the wings of Neuroptera, the ink-bags of Sepiadæ; now questioning the great English Botanist on the reticulated stems of Cycadeoideæ, and fathoming the mind of Owen on the little Marsupialia of Stonesfield, or inviting the eagle glance of Cuvier on the serrated teeth of *Megalosaurus Bucklandi.*

So passed the life of this man, strong in mind and strong in body; working hard and setting others to work; gathering and giving knowledge; a patient student, a powerful teacher, a friendly associate; a valiant soldier for Geology in days when she was weak, an honoured leader in her hour of triumph.

Perhaps of all the varied marks of respect which were heaped upon him by the learned societies in all parts of the world, none yielded him higher gratification than that which threw a ray of splendour over his latest appearance at the meetings of the Geological Society. For there, in February 1848, he received from the hands of Sir H. T. De la Beche, with very appropriate expressions, the Wollaston Medal, which is the highest mark of honour known in Geological Science—an honour which would, doubtless, long before have been paid to him, but for the frequency of his election to office in that Society*. In the reply of Dr. Buckland to the Address of the President, we find expressions such as could only be uttered by a geologist convinced of the grand destiny of his science, and conscious of his own right to be remembered among the authors of "discoveries whose names are inscribed on the annals of the physical history of the globe." And these are followed by words which embody a humble confession of the comparative littleness and incompleteness of all human knowledge—words too prophetic of the approaching close of his own valuable and honourable career,—for within two short years that apparently indefatigable mind ceased from its labours, and only the form of Buckland survived till the 15th of August, 1856.

Dr. William Frederick Chambers died of paralysis in December 1855, aged 69 years. Prior to his retirement from active

* He was President for the second time in 1840–1841.

life on account of the disorder which finally proved fatal, he had for many years had the most extensive practice as a physician of any in London. The mental character to which he owed this distinction is interesting as a subject of psychological study, and valuable as an example and encouragement to those who desire to lead a similar life of usefulness. His intellectual powers were not of that order to which it is usual to apply the term "genius:" no original discovery, no striking innovation marked his career. Nor was he a man of very sparkling talent: there was nothing that could be called "brilliancy" in his thought, his writing, or his mode of action. What he possessed in an eminent degree was, wisdom, judgment—that peculiar balance of faculties which enables a man to think soundly and to be a safe adviser and guardian. The circumstances of his life had helped to give this form to his character. He had received his public education at Westminster and Cambridge, where the studies are such as to cultivate in an equal degree the imaginative and scientific faculties. The postponement of his entrance on special professional studies till he was three-and-twenty years of age, enabled him to bring to these studies, when he did engage in them, a fully-formed mind, and so to escape the danger often arising from crude prejudices acquired in early studentship. His election, at the age of thirty, as Physician to St. George's Hospital, kept him afterwards closely to the duties of practical life, from which he was never distracted by special scientific inquiries; and accordingly his lectures on the practice of medicine and the lectures on cholera, which at the request of his colleagues he gave in 1833, with the addresses which he delivered as President of the Medical and Chirurgical Society in 1846 and 1847, constitute the bulk of what he has published to the world. He became a Fellow of the Royal Society in 1828, and through life was a conspicuous illustration of the intimate connexion between sound science and practical usefulness.

Sir Alexander Crichton, second son of Mr. Alexander Crichton of Woodhouselee and Newington in Mid Lothian, was born in Edinburgh on the 2nd of December, 1763. He received his elementary education in his native town, and afterwards matriculated in its University. He was placed at an early age with Mr. Alexander Wood, a surgeon of eminence in Edinburgh. At the expiration

of his apprenticeship, in 1784, Mr. Crichton came to London to prosecute his studies, more especially anatomy, and the following spring he went to Leyden, in company with Mr. Robert Jackson, who became afterwards so favourably known by his writings on subjects connected with military surgery. Though Mr. Crichton had been brought up with the view of prosecuting surgery as his profession, he thought it advisable to submit himself to the necessary examinations before the Professors of Leyden for the degree of M.D., which he obtained in July 1785.

After passing a short time in Holland, he proceeded to Paris to perfect himself in the French language, and to avail himself of the facilities afforded in that city for advancement in every department of medical knowledge.

Leaving Paris in the summer of 1786, Dr. Crichton studied successively at Stuttgardt, Vienna, and Halle, residing, during his stay at the last-named University, in the house of Professor Meckel, the second celebrated anatomist of that name. He then passed some time in Berlin and in Göttingen, where he remained till September 1788. Returning from Germany, where he had spent three years in the acquisition of medical and scientific knowledge, Dr. Crichton established himself in London as a surgeon, and became a member of the Corporation of Surgeons in May 1789. But not liking the operative part of the surgical profession, he withdrew from that body on May 1, 1791, and became a licentiate of the Royal College of Physicians on the 25th of June, 1791; shortly after which he was appointed Physician to a large Dispensary in Featherstone Buildings, Holborn. There, in conjunction with Dr. Bradley, he formed a "Clinical Institution," upon a plan similar to that followed at the University of Göttingen, and delivered Lectures upon the most remarkable and instructive cases which presented themselves. About 1796 Dr. Crichton was elected Physician to the Westminster Hospital, and during his connexion with that institution he was in the practice of delivering three courses of lectures; viz. on Chemistry, on Materia Medica, and on the Practice of Physic. In 1798 he published his work on Mental Derangement, which gained him reputation at home and abroad; and having now attained a high professional position, he was appointed Physician to the Duke of Cambridge. In 1803 Dr. Crichton was invited to become physician

in ordinary to His Imperial Majesty Alexander I. of Russia. Having accepted this appointment, he was kindly received in St. Petersburgh, and soon gained the full confidence and esteem of the Emperor and the several members of the imperial family. In the course of a few years he was also appointed to the head of the Civil Medical Department, in which capacity he was much consulted by the Empress Dowager, in the construction and regulation of many institutions which owe their origin to her active charity and watchful superintendence.

Dr. Crichton's exertions to mitigate the horrors of an epidemic which was devastating the south-east provinces of Russia in 1809 were acknowledged by the Emperor, who conferred on him the title of Knight Grand Cross of the Order of St. Vladimir, Third Class. In 1814 His Imperial Majesty bestowed on him that of the Second Class for his long and faithful services, and as "Médecin en chef pour la partie Civile." Having obtained leave of absence on account of the state of his health, he returned to this country in the spring of 1819. The following year, however, he was recalled to attend the Grand Duchess Alexandra (the present Dowager Empress), whom he accompanied, on her convalescence, to the court of Berlin, where he stayed a short time, and then returned to his family. On the 27th of December, 1820, His Majesty Frederick William III. created him Knight Grand Cross of the Red Eagle, Second Class. In 1821 Dr. Crichton was knighted by His Majesty George IV., and obtained the royal permission to wear his foreign orders. The late Emperor Nicholas I. also marked his sense of the services of Sir Alexander Crichton by bestowing upon him the additional title of Knight Grand Cross of the Order of St. Anne, in August 1830.

Dr. Crichton married, in 1800, Frances, daughter of Mr. Edward Dodwell, of West Moulsey. He was one of the oldest members of the Linnean and Royal Societies, having been elected a member of the first in 1793, and of the latter in 1800. He was member of the Imperial Academy of Sciences of St. Petersburgh, and of the Imperial Society of Naturalists of Moscow, and Corresponding Member of the Royal Society of Sciences of Göttingen, of the Royal Institute of Medicine at Paris, and of many other societies. His writings were the following:—

A Translation of Dr. J. F. Blumenbach's Essay on Generation. 1792.

An Inquiry into the Nature and Origin of Mental Derangement, &c. London, 1798.

An Account of some Experiments made with the Vapour of Boiling Tar in the Cure of Pulmonary Consumption. 1817.

On the Treatment and Cure of Pulmonary Consumption, and the Effects of Boiling Tar on that Disease. 1823.

Commentaries on some Doctrines of a dangerous tendency in Medicine, and on the general principles of Safe Practice. 1842.

George James Guthrie was born in London on the 1st of May, 1785, and died on the 71st anniversary of his birthday. He was descended from an old and respectable Forfarshire family, one of whom, his great-grandfather, married an Irish lady, and settled in her country. His father, a manufacturer of plaister and other surgical materials, raised himself from poverty to considerable wealth; but, late in life, was again impoverished, and left his son at an early age to seek and work his own way in the world. He was educated in boyhood by an emigrant French gentleman, M. Noel; and, when thirteen years old, he was apprenticed to the medical profession, at the instance of Mr. Rush, one of the Army Medical Board. For a time he received his chief instruction from Dr. Hooper, one of the most active pathologists of the day. In June 1800 Mr. Rush appointed him an hospital-assistant at York Hospital (a military hospital which then stood on part of the site of Eaton Square); and in the following winter he assisted Mr. Carpue in teaching anatomy. In the beginning of 1801 he was to have been removed from his appointment, with all the other hospital-assistants who had not been examined at the College of Surgeons; and it gave proof of the success with which he had already studied, and promise of the spirit which marked his after-life, that he immediately offered himself for the examination. He passed, and obtained his diploma at the College in February 1801; and in the next month, though not yet sixteen, was appointed assistant-surgeon to the 29th Regiment, with which, from 1802 to 1807, he served in North America.

In 1808, Mr. Guthrie having risen to the surgeoncy of his regiment, accompanied it to Spain; and from that time to the end of the Peninsular war (with the exception of a period of severe illness in 1810), was engaged in the most active service. He had a chief share in the charge of the wounded at the battles of Roliça and Vimiera;

at the taking of Oporto; at Talavera and Albuera; at the sieges of Olivença and Badajos; at Ciudad Rodrigo, Salamanca, and Toulouse. In these fields of action he justly earned the highest reputation among the British military surgeons of his time; and all his writings prove that they were to him fields not only of action but of study.

In September 1814, Mr. Guthrie was placed on half-pay, and commenced private practice in London. After the battle of Waterloo, he spent a few weeks at the military hospitals at Brussels and Antwerp, studying chiefly those points of practice on which his Peninsular experience had left him uncertain. Returned to London, he commenced lecturing on surgery in 1816, and was appointed surgeon to the Westminster Ophthalmic Hospital, the establishment of which was chiefly due to his exertions. In 1826 he was elected assistant-surgeon, and in 1827 full surgeon to the Westminster Hospital. In the last-named year, also, he was elected a Fellow of the Royal Society. In the College of Surgeons, he became a Member of the Council in 1824, President in 1833, 1842, and 1854, and during five years was Professor of Anatomy and Surgery. [Nearly all the foregoing statements are derived from an evidently authentic biography of Mr. Guthrie in the 'Lancet' of June 15, 1850.]

It would be very difficult to form a catalogue of Mr. Guthrie's publications, for he was always active in publishing his knowledge and opinions on all the questions which he had had opportunities of studying. His chief works are,—"On Gun-shot Wounds of the Extremities requiring Amputation" (1815); "Lectures on the Operative Surgery of the Eye" (1823); "On the Diseases and Injuries of Arteries" (1830); "On the Anatomy and Surgery of Herniæ" (1833); "On the Anatomy and Diseases of the Urinary and Sexual Organs" (1836); "On Injuries of the Head, affecting the Brain" (1842); "On Wounds and Injuries of the Abdomen and the Pelvis" (1847); "Commentaries on the Surgery of the War in Portugal, Spain, France, and the Netherlands," of which the last edition was published in 1855, and comprised additional observations on the Surgery of the Crimean war.

Enterprise, activity, and self-reliance were the chief characteristics of Mr. Guthrie's mind. His intellect was acute and clear; his

habits orderly and business-like; his constitution naturally robust, and, till he reached old age, capable of great exertion and endurance. These qualities, in circumstances so favourable to their exercise as those of the Peninsular war, quickly and justly placed him in the first rank of military surgeons, and accomplished a large amount of good in the medical department of the Army. In after-life, the same qualities, strengthened by success, ensured great influence for what he taught, gained for him a large private practice in surgery, and made him a man much to be considered in all the questions of professional interest in which he was engaged. His influence on the progress of medical science in his own time was that of an earnest advocate and an attractive teacher of whatever appeared simple and straightforward in practice, and of all surgical doctrines that professed to be based upon correct anatomy. In the future history of surgery, he will be remembered for his advocacy of the use of nitrate of silver in purulent ophthalmia, of large incisions in phlegmonous erysipelas, of acid escharotics in sloughing phagedæna, and for the skill and boldness of his treatment of gun-shot wounds. But, especially, his name will probably be always mentioned with honour for his maintenance of the general necessity of tying wounded arteries at the very seat of injury, above and below the opening. The usual practice had been to tie the artery at some convenient part above the wound, on the assumption that the arrest or diminished force of the circulation would allow the firm closure of the wound, as it does the obliteration of an aneurismal sac. Few things in modern surgical works are equal in strength and clearness to the chapters in which Mr. Guthrie proved the error of such an assumption, and the advantages of his own mode of practice. In anatomy, his best work was the bringing to general knowledge the *musculi compressores urethræ*, which, though described by Santorini, had nearly ceased to be recognized. In the medical department of the Army, his influence for good was undoubtedly considerable. It may be difficult to enumerate the improvements that were due to him; but, as the last edition of his best work—the 'Commentaries on the Surgery of the War'—will prove, he was to the very end of life urgent in promoting the efficiency of military hospital-establishments, and in maintaining the reputation of the medical officers of the Army.

Daniel Sharpe was born in London in 1806. His mother, who died a few weeks after his birth, was sister to Samuel Rogers the poet. He was educated at Walthamstow, and as a boy early showed a taste for the study of natural history, but he did not commence seriously to work at geology till he was admitted a Fellow of the Geological Society in June 1829. In the same year he gave his first memoir to the Society, "On a new species of Ichthyosaurus, &c."—*I. grandipes*—which, however, it afterwards appeared, had been previously described by Conybeare, under the name of *I. tenuirostris*.

Throughout the greater part of his life Mr. Sharpe was actively engaged as a merchant, and his business connexion with the wine-growing districts of Portugal occasionally leading him there, in 1832, 1839, 1848 and 1849 he gave to the Geological Society a series of memoirs on the rocks in the neighbourhood of Lisbon and Oporto. The first is a mere sketch of the general arrangement of the Tertiary and Secondary rocks by a young and intelligent geologist;—the second, on the same subject, is fuller and more definite, but not sufficiently complete in the determination of fossils to fix the precise age of the strata described. It contains, however, in an appendix, some observations of great value on the comparative effects of the great earthquake of 1755 on the strata on which Lisbon stands. The destructive effects of this shock were chiefly confined to the area occupied by the soft tertiary beds, while the buildings erected on the more solid Hippurite limestone and chalk escaped entirely. The line of division between the shattered and entire buildings corresponded precisely with the boundaries of the strata. This subject has since been elaborated by Mr. Mallet in his Reports on Earthquakes to the British Association. In his third memoir Mr. Sharpe describes the granitic, gneissic, clay-slate and coal-bearing rocks of Vallongo near Oporto. The clay-slate he proved by its fossils to be of Lower Silurian age, and his sections show that the strata bearing anthracitic coal underlie the slate, and rest on gneiss pierced by granite. He thence concluded that the coal is of Lower Silurian age. In the present state of knowledge regarding that country, it is impossible to deny that this may be the case, but it must be remembered that the few remains of plants discovered in these strata are considered by palæontologists to present characters indicative of "Carboniferous"

age; and even those geologists who most strenuously support the so-called uniformitarian doctrines, incline to attribute the peculiar position of the coal to one of those great inversions of the strata so frequent in highly disturbed districts of all ages, from Palæozoic up to Tertiary times.

The fourth paper commences with a succinct sketch of the general geology of Portugal, and goes on to define the limits of the secondary rocks north of the Tagus, both by stratigraphical and palæontological evidence. Long before this paper was read, Mr. Sharpe had acquired much critical skill and knowledge as a palæontologist, and on palæontological principles he now established the existence of Cretaceous and Jurassic rocks in the country described. The whole formed an excellent sketch of a hitherto undescribed country, and up to this date British geologists are chiefly indebted to these memoirs for the knowledge they possess of a land where the science is almost uncultivated.

Between 1842 and 1844 Mr. Sharpe gave four memoirs to the Geological Society on the Silurian and Old Red Sandstone Rocks of Wales and the North of England, territories previously chiefly illustrated by the labours of Professor Sedgwick. The first of these is "On the Geology of the South of Westmoreland." Part of this paper describes the range of the Coniston limestone. Mr. Sharpe identified it by its fossils as forming part of the Lower Silurian series, but did not determine its actual horizon. In 1839 Mr. Marshall placed it on the parallel of the Caradoc sandstone, which determination the researches of later geologists have sustained.

Mr. Sharpe also pointed out the unconformity of the Upper on the Lower Silurian rocks of the area; and in describing the passage of the Ludlow rocks into the Old Red Sandstone, he correctly infers that the Tilestones of South Wales should be withdrawn from the base of the Old Red Sandstone and classified with the Ludlow rocks, to which their fossils unite them. At a later period of the same year he produced a memoir "On the Bala Limestone, and other portions of the Older Palæozoic Rocks of North Wales." Up to this date it was believed that at Bala and elsewhere there was a great thickness of fossiliferous Upper Cambrian rocks below the Lower Silurian strata. Mr. Sharpe maintained that this was an error, and that both stratigraphically, and by their fossils, the Bala rocks were the equi-

valents of the Llandeilo flags and Caradoc sandstone. This sagacious determination has since been confirmed by Mr. Salter as regards the Caradoc sandstone; the fossils of Bala and the typical Caradoc sandstone of Sir Roderick Murchison in Shropshire being the same.

The more elaborate paper of 1844 is accompanied by a geological map of North Wales, and is less happy. Mr. Sharpe's genius chiefly lay in the palæontological determination of the age of rocks, and, in this case at least, the time he allowed himself to map North Wales was too short for the satisfactory elucidation of the problems he proposed to solve.

Pursuing at intervals these subjects, Mr. Sharpe produced in 1847 an elaborate analysis and comparison of the Silurian fossils of North America (collected by Sir Charles Lyell) with those of Great Britain, and confirmed the views entertained by the American geologist, Mr. Hall, that the American Silurian strata, like the British, consist of two great divisions, viz. Upper and Lower.

While engaged in these investigations, Mr. Sharpe's attention was drawn to the subject of slaty cleavage and foliation, which affects the more ancient rocks of Devonshire, Wales, the North of England, the Highlands of Scotland, and Mont Blanc. In 1846, 1848, 1852 and 1854 he produced four memoirs on these subjects, the two first and the last of which are published in the Journal of the Geological Society, and the third in the Philosophical Transactions. These questions had previously been made the subject of special investigation by Professor Sedgwick, Mr. Darwin, and Professor Phillips. It has been said that from imperfect data Mr. Sharpe generalized too largely; and though this may be the case, an attentive perusal of the memoir of 1846 proves that in some important points he materially advanced the subject at that date in the direction to which the labours of Mr. Sorby have since tended. He attributes the cleavage of rocks, and consequent distortion of fossils, to pressure perpendicular to the planes of cleavage, and asserts that rocks are expanded along the cleavage planes in the direction of the dip of the cleavage. In the communication of 1848, the doctrine that pressure is the cause of cleavage is still more distinctly insisted on, and remarkable instances are given in which *pebbles* were observed which appeared to have been compressed and elongated in the planes of cleavage. He also recognizes the fact, since so beautifully explained by Mr. Sorby,

that the fine particles composing the slaty rocks are arranged lengthwise in the direction of the cleavage planes, and he attributes bends in the cleavage in its passage from one bed to another, to beds of different lithological character offering different degrees of resistance to pressure. The idea that cleavage may be due to crystalline action, he altogether repudiates. The two last of the series, published in 1852 and 1854, describe respectively the cleaved and foliated rocks of Scotland and Mont Blanc, and are chiefly devoted to the development of his theory of the great "cylinders" or arches, in which he asserted that the laminæ of cleaved and foliated rocks lie. In these memoirs he made no advance beyond his previous ideas, for he attributed the formation of cleavage and foliation to the same cause; and though he indicated the fact, he gave no explanation of the reason of the occurrence of planes of cleavage and foliation in arched lines, a subject that has since in part been acutely treated of by Mr. Sorby, and of which the full explanation seems not far distant.

Besides these memoirs, Mr. Sharpe contributed to the Geological Society various papers on special subjects:—"On the Quartz Rock of MacCulloch's Map of Scotland;" "On the Southern Borders of the Highlands of Scotland;" and various palæontological communications: "On the genus *Trematis*;" "On *Tylostoma*, a new genus of Gasteropods from the Cretaceous beds of Portugal;" "On the genus *Nerinæa*;" and a note on the fossils of the Boulonnais, appended to a paper by Mr. Austen on that district. He also furnished several parts of a Monograph to the splendid publications of the Palæontographical Society, "On the Fossil Remains of the Mollusca found in the Chalk formation of England," and on this important work he was still engaged when he met with the unhappy accident that caused his untimely death.

Such is a brief outline of some of the scientific labours of Daniel Sharpe—a man, whose mind alike powerful and active, and well cultivated, urged him successfully to grasp and make his own a wider range of subjects than many geologists dare to attempt. Neither should it be forgotten that all the while he was unceasingly engaged in mercantile pursuits, and it was only during brief intervals of leisure, when more imperative labours were over, that he accomplished what many would consider sufficient work for their lives. And it is not in geology alone that he is known and appreciated:

philologists and ethnologists equally esteemed him. With marvellous versatility of talent he grappled with the ancient Lycian inscriptions, brought home by Fellows, Forbes and Spratt, and revealed the secrets of an unknown tongue written in an unknown character.

In debate he was clear, keen, severely critical, and at times somewhat sarcastic, occasionally alarming to an opponent unaccustomed to his style; but those who knew him best were well aware that an unvarying fund of kindly good humour lay beneath, and that if he hit his adversary hard, no man than himself more rejoiced in a harder blow in return. His private life was full of unostentatious benevolence. In conversation with his familiars he was intelligent, lively, and quick in perception, and his attached friends of the Geological Club, of which he lately was President by virtue of his office as head of the Society, will long mourn his loss, and miss the quaint humour and quiet laugh that so often helped to animate their board.

Mr. Sharpe was a Fellow of the Linnean, Zoological, and Geological Societies. In 1853 he became Treasurer of the Geological Society, and on the retirement of Mr. Hamilton was elected its President in 1856. In 1850 he was elected a Fellow of the Royal Society. On the 20th of last May, while riding near Norwood, he was thrown from his horse, and sustained a fracture of the skull. In a few days he so far recovered as to be able to recognize the relations that were admitted to his chamber, and his numerous friends rejoiced in the prospect of his speedy restoration; but a sudden relapse succeeded, and he died on the 31st of May, sorrowed for by all who knew his worth.

James Meadows Rendel was born in 1799, at a village on the borders of Dartmoor, in Devonshire; his grandfather, Mr. Meadows, F.R.S., was a well-known architect, and his father, who was a county surveyor and farmer, was a man of ability, excellent common sense and determination of character, qualities which descended to the son, whilst to his mother, who was a woman of considerable acquirements, he owed the rudiments of his early education.

After being practically instructed in the executive part of his profession, he went to London and obtained an engagement under Mr. Telford, by whom he was employed on the survey and experiments for the proposed suspension bridge over the Mersey, at Runcorn, and

subsequently on the survey and construction of roads in the north of Devon, where the difficulties he had to contend with contributed much to create that self-reliance so useful to him in his subsequent career. At that period he was introduced to the Earl of Morley, who discovering the latent talents of the young engineer, then scarcely twenty-five years of age, confided to him, with the approval of Mr. Telford, the construction of a cast-iron bridge across the Lary, an arm of the sea within the Harbour of Plymouth. This bridge, consisting of five elliptical arches, was, with the exception of that of Southwark, the largest cast-iron structure of the kind in the kingdom. Its construction, in which Mr. Rendel was engaged between 1824 and 1827, presented many difficulties demanding considerable skill and decision on the part of the engineer; but these difficulties were successfully overcome, and for the account of this work the Telford Medal of the Institution of Civil Engineers was awarded to him. About this period he designed and executed the Boucombe Bridge, where hydraulic power was for the first time applied to the machinery for working swing bridges.

Soon after the completion of the Lary Bridge, Mr. Rendel settled in Plymouth, and there exercised his profession with great activity, being engaged in surveying and reporting upon nearly all the harbours in the South-west of England, and executing the works at a large number of places, acquiring that mastery over Hydraulic Engineering on which his fame will chiefly rest. He was extensively employed by the Exchequer Loan Commissioners; in many cases executing the works thus authorized.

In the year 1831 he introduced a new system of crossing rivers by means of Floating Bridges worked by steam power; they were applied at Saltash and at Torpoint, on the river Tamar, and subsequently at Southampton and Portsmouth; but the rapid progress of the railway system prevented the further development of this useful invention, for which the Telford Medal was awarded. Descriptions of the structure of these bridges, as well as of that over the Lary, were published in the Transactions of the Institution of Civil Engineers.

The repairs of the Montrose Suspension Bridge, after its fall, were confided to him, and he there introduced the system of imparting that rigidity to the platform of the roadway which is now admitted to be so essential to the safety of these structures. He was also

engaged in the surveys for a railway between Exeter and Plymouth, but the necessary funds not being provided, the scheme was abandoned, and the district eventually falling under the control of the Great Western Railway Company, the present line of railway was constructed by Mr. Brunel.

In the year 1838 Mr. Rendel removed to London, where he was soon consulted upon many important works, and was engaged in the chief parliamentary contests of that remarkable period in the history of engineering. About this time he designed the Pier at Millbay, where he introduced the system of construction since employed with so much success at the harbours of Holyhead and Portland. Engagements poured in fast upon him, and his career was for the next few years one of unceasing activity, chiefly in the construction of Harbours or Docks, and the improvement of Rivers and Estuaries.

In the year 1843, the projected construction of Docks at Birkenhead, in Cheshire, of such an extent as to create a formidable rival to Liverpool, brought Mr. Rendel very prominently before the world; and the protracted contests on this subject will be long remembered in the history of Parliamentary Committees, for the ability with which he defended his positions; and the evidence given by him and other Engineers, as now collected, forms a valuable record of the state of engineering practice. The almost incessant labour, and the mental anxiety inseparable from this undertaking, were more than even his powerful constitution could support, and it is feared that they tended to shorten his valuable life.

The daring project of constructing a dock at Great Grimsby, by projecting the works far out upon the mud-banks of the River Humber, was next successfully accomplished; and he commenced the two great works which alone suffice to hand down his name to posterity, beside those of Smeaton, Rennie, and Telford,—the Harbours of Refuge of Holyhead and Portland; both these works were conceived with the largest views, and have been carried on with great rapidity. In both cases the system was adopted of establishing timber stages over the line of the jetties and depositing the large and small stones together, as they came from the quarries, by dropping them vertically from railway waggons into their positions; thus bringing up the mass simultaneously to above the level of the sea. In this manner as much as 24,000 tons of

stone have been deposited in one week, and to supply this vast demand, monster blasts of five or six tons of gunpowder were frequently employed. These two great works are advancing very satisfactorily; and it is worthy of remark, that although the severe storms which have repeatedly occurred on the exposed coasts where they are situated, have done some injury to portions of the stages, and of the temporary works, at Holyhead—where the piles were not shod with Mitchell's screws, which proved so successful at Portland—not a stone would appear to have been carried away from the jetties; and the success of the system may be said to be complete, in spite of the sinister predictions which prevailed before it was tried.

Among the other works upon which Mr. Rendel was engaged, should also be mentioned the constructions on the River Lea, and the improvements of the Nene river; the latter an undertaking of considerable difficulty, and not yet completed. He was also employed by the Exchequer Loan Commissioners to report upon the drainage and other public works in Ireland.

He was less engaged in railways than in hydraulic works, but in England he executed the Birkenhead, Lancashire, and Cheshire Junction Line, and in India he had the direction of the "East Indian" and the "Madras Railways," the former projected by Mr. (now Sir Rowland) Macdonald Stevenson, as the first of the vast system now in progress which will doubtless exert a mighty influence on the future destiny of the Indian Empire. The Ceylon and the Pernambuco lines were also under his charge.

It would unduly extend this sketch to notice in particular the various hydraulic works upon which Mr. Rendel was engaged, as there was scarcely a harbour or river of importance in the kingdom with which he was not connected in some capacity. His advice was also sought by foreign countries; and he was engaged to report upon works for the Brazilian, the Prussian and the Sardinian governments, and was nominated by the Viceroy of Egypt a Member of the International Commission for considering the construction of the Canal across the Isthmus of Suez.

He was a man of great energy, clear perception, and correct judgment; his practical knowledge was well directed, and he knew how to make good use of the scientific acquirements and skill of all whose services he engaged. His evidence before Parliamentary Committees

was lucid and convincing,—seldom failing in carrying his point; and his Reports on Engineering works are distinguished by the clearness and correctness of his views and the fearless expression of his opinion, and are so well conceived and drawn up, that it may be hoped they will be given to the world. With these qualities, which were fully appreciated, it need scarcely be added that he rose rapidly to a very high position in his profession. He became a Fellow of the Royal Society in 1843, and was elected upon the Council; he was a very early Member of the Institution of Civil Engineers, having joined it in 1824. He had been for the last sixteen years upon the Council, and held the post of President during the years 1852 and 1853.

Mr. Rendel was as amiable and kind in private life as he was energetic and firm in public, and his decease, which occurred on the 21st of November, 1856, cast a gloom over the whole of the profession of which he was a brilliant ornament.

Rear-Admiral Frederick William Beechey, V.P.R.S., P.R.G.S., was the second son of the eminent painter Sir William Beechey, R.A. He was born on the 17th of February, 1796, and his godfather was H.R.H. the Duke of Clarence, afterwards William the Fourth.

When only ten years of age, he was sent to sea on board the 'Hibernia,' under the immediate patronage of Lord St. Vincent; and it may be justly presumed that the well-known sentiments and practice of that stern disciplinarian had no small influence in forming the young seaman's professional character, and especially in inspiring that unremitting activity and devotion to duty for which in after-life he was conspicuous.

After a short time passed on board the 'Minotaur,' young Beechey went with the gallant Sir Sidney Smith, in the 'Foudroyant,' to Brazil. In 1811 he served on board the 'Astræa,' frigate, Captain Schomberg, and as an ardent youth of fifteen, and captain's aid-de-camp, shared in those long and gallant actions off Madagascar, in which his ship bore a part, and which ended in the capture of two French frigates, and the surrender to the British of the Fort and Settlement of Tamatave. After this he remained a short time on the home station, and in 1815 was on board the 'Vengeur' in the

Expedition against New Orleans, and took part in the operations on the Mississippi in support of the general attack on the American lines. While on board the 'Tonnant' he received his commission as Lieutenant, dated 10th of March, 1815, and in the following year was appointed to the 'Niger,' frigate, on the North-American station.

In January 1818, when he had been nearly twelve years in active service, Lieutenant Beechey commenced his career as an Arctic voyager, under his friend the late Sir John Franklin, in the 'Trent,' which vessel was associated with the 'Dorothea,' Captain Buchan, in an attempt to discover a northern communication between the Atlantic and Pacific Oceans. In this Expedition many important scientific observations were made, and much useful information was gained respecting the coast of Spitzbergen and the sea adjacent; and it is worthy of note, that, with the exception perhaps of some early voyages by the Dutch, this, and the contemporaneous voyage of Captain Ross to Baffin's Bay, were the first in which very deep soundings were obtained; mud and stone being brought up from the bottom at a depth of more than a thousand fathoms, by means of the contrivance called "deep-sea clamms." A narrative of this Expedition was published by Lieut. Beechey in 1843, and has often been favourably noticed by foreign as well as English journals. On his return from this voyage, Beechey and his late commander, Lieutenant Franklin, volunteered to attempt to reach the North Pole by a journey over the ice; and with this view they submitted to the Admiralty a plan of proceeding, which was afterwards adopted, and to a great extent carried out by Sir Edward Parry in 1827.

In January 1819 Lieutenant Beechey was appointed to the 'Hecla,' under the command of Lieutenant (afterwards Sir Edward) Parry, with whom he penetrated to the western longitude of 113° 55′, within the Arctic Circle, wintered at Melville Island, and shared the Parliamentary reward of £5000.

In these Arctic expeditions the merits of Lieut. Beechey as an able and vigilant officer, a skilful astronomical observer, and an accomplished draughtsman, met with general recognition; but it was also in these memorable voyages,—through hardships and exertions, disregarded as usual at the time by a young and ardent

adventurer,—that the foundation was laid of disease, which was destined to terminate prematurely his valuable life.

In January 1821 Lieut. Beechey was appointed to the 'Adventure,' then commanded by Captain (now Rear-Admiral Smyth); and in November of the same year, he was detached from the ship in charge of an Expedition along the northern shores of Africa, and round the greater Syrtis towards Egypt. In this Expedition Lieut. Beechey surveyed all the coast between Tripoli and Derna, explored the country and antiquities of the Cyrenaica, and determined the sites of the five cities of the Pentapolis. In this service he was accompanied by his brother Mr. Henry Beechey, whose intimate knowledge of the language, as well as of the manners and customs of the inhabitants, acquired whilst with Messrs. Salt and Belzoni during their researches among the Pyramids, was of the greatest assistance. An account of this interesting Expedition was published in 1828.

While on this service Lieut. Beechey was promoted; and in January 1825 was appointed to command the 'Blossom' on a voyage to the Pacific and Arctic Oceans, intended to co-operate with the Polar Expeditions under Parry and Franklin. During this voyage he passed twice through Behring's Strait, and explored the north-west coast of America, 120 miles beyond the farthest point of Cook, attaining to lat. 71° N. and long. 156° W.,—a spot scarcely 150 miles from the extreme point reached by Franklin overland. The intervals between the seasons available for Arctic navigation were passed in visiting many parts of the Pacific Ocean, where he discovered and made surveys of several new islands, and contributed largely to the hydrography and general knowledge of those regions. In 1827 Commander Beechey was promoted to the rank of Captain, and in the following year returned to England with his ship, in which, notwithstanding her dull sailing, he had traversed 73,000 miles, and rendered valuable service to navigation and general science. In 1831 he published a narrative of this voyage, replete with interesting and valuable information, and evincing throughout the high qualifications of the author for the conduct of such an Expedition.

In 1835 Captain Beechey was appointed to the 'Sulphur,' to undertake another voyage of exploration and survey, but the inroads which engrossing pursuits and incessant mental activity had even

then made in his health, obliged him to return home from Valparaiso in 1836.

After an interval of leisure passed with his family, his ardent desire of active employment induced him, in 1837, to accept an appointment to conduct various surveys of the Irish Sea and the Western Coasts of England and Wales. Among the important results of these surveys was a series of observations on the Tides around the British Islands, which formed the subject of a paper read before the Royal Society, and published in the 'Philosophical Transactions;' and the estimation of Captain Beechey's labours by the Society may be judged of by the fact, that the Council requested the Admiralty to afford him the means of continuing his inquiries.

For some years subsequently, Captain Beechey was chiefly employed in continuing these services, and in reporting to Government on harbours, stations for marine postal communication, and other public undertakings of a like description. In 1848 and 1849 he attended Her Majesty in her visits to Scotland and Ireland, in charge of the pilotage of the Royal Squadron; and in 1851 he was appointed Aide-de-camp to the Queen.

The Government having resolved to establish the Marine Department of the Board of Trade, Captain Beechey was chosen to assist in its organization, and in this important and very onerous duty, to which he was appointed in 1850, he laboured incessantly, day and night, to the great detriment of his health. In 1853 he was selected to take part in the "Meteorological Conference" held at Brussels, for the purpose of devising a great scheme of international co-operation in obtaining meteorological observations at sea, which was to be promoted and superintended on the part of England, by the Marine Department of the Board of Trade. In the following year he obtained his Flag as Rear-Admiral on the active list.

Admiral Beechey had long been Fellow of the Astronomical, Geological, and Geographical Societies, and had contributed valuable communications to these bodies. He was elected into the Royal Society in 1824, and in 1854 was nominated a Vice-President; at the time of his decease he was President of the Royal Geographical Society.

A short time before Admiral Beechey's acceptance of the Presidency of the Geographical Society, he was attacked by severe illness,

the consequence of hardships endured and bodily and mental powers overstrained, in the cause of science and of that profession to which through life he was so earnestly devoted. The Chair was, however, left vacant in the hope of his speedy recovery, and on this hope being to some extent fulfilled, he was unanimously elected, and entered with his usual zeal on the duties of that honourable position. This was in 1855: in May of the following year he delivered his Presidential Address at the Anniversary Meeting of the Society, and this was his last conspicuous public duty. His health, notwithstanding his partial recovery, was permanently broken, and a renewed attack of illness terminated his useful life on the 29th of November, 1856, in the sixty-first year of his age.

It may be long before we are called on to record public services of nearly half a century more ably, earnestly, and usefully performed than those of Admiral Beechey. On his private virtues this is not the occasion to expatiate; it will suffice here to say that he was a sincere Christian, and a gentleman in the best sense of the term, and that through life he was supported by a firm trust in Providence, and actuated by a single-minded determination at all times to do his duty.

Admiral Beechey married, in 1828, Charlotte, youngest daughter of Colonel Stapleton, of Thorpe Lee, Surrey, and has left several children.

On the motion of Sir Benjamin Brodie, seconded by the Rev. Professor Powell, the best thanks of the Society were voted to the President for his excellent address, and his Lordship was requested to permit the same to be printed.

The Statutes relating to the election of Officers and Council having been read, and Dr. Gray and Professor Bell having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were collected.

The following Noblemen and Gentlemen were reported duly elected Officers and Council for the ensuing year:—

President.—The Lord Wrottesley, M.A.

Treasurer.—Major-General Edward Sabine, R.A., D.C.L.

Secretaries.—William Sharpey, M.D.; George Gabriel Stokes, Esq., M.A., D.C.L.

Foreign Secretary.—William Hallows Miller, Esq., M.A.

Other Members of the Council.—James Moncrieff Arnott, Esq.; William Benjamin Carpenter, M.D.; Arthur Cayley, Esq.; The Very Rev. The Dean of Ely; William Fairbairn, Esq.; Arthur Farre, M.D.; William Robert Grove, Esq., M.A.; Joseph Dalton Hooker, M.D.; William Hopkins, Esq., M.A.; William Allen Miller, M.D.; Lyon Playfair, Esq., C.B., Ph.D.; Rev. Bartholomew Price, M.A.; Sir James Clark Ross, Capt. R.N., D.C.L.; Rear-Admiral W. H. Smyth, D.C.L.; John Stenhouse, LL.D.; John Tyndall, Esq., Ph.D.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Honorary.	Foreign.	Having compounded.	Paying £2 12*s.* annually.	Paying £4 annually.	Total.
December 1, 1855..	9	49	385	13	273	729
Since elected	......	+2	+3	......	+12	+17
Re-admitted	......	......	......	......	+1	+ 1
Since compounded..	......	......	+4	......	−4	
Withdrawn	......	......	......	−2	......	− 2
Since deceased	......	−1	−16	−1	−7	−25
November 30, 1856	9	50	376	10	275	720

Statement of the Receipts and Payments of the Royal Society between November 30, 1855, *and December* 1, 1856.

	£	s.	d.
Subscriptions and Compositions	1643	4	0
Rents	186	15	2
Dividends on Stock	1092	7	11
Sale of Transactions, Proceedings, &c.	297	16	0
Sale of 3 R. and 20 P. Acton Estate	360	0	0
Bequest, Henry Lawson, Esq., F.R.S.	200	0	0
	£3780	3	1

Estates and Property of the Royal Society, including Trust Fund.

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £116 16*s*. per annum.

Estate at Acton, Middlesex (28 A. 0 R. 21 P.), £60 0*s*. 0*d*. per annum.

Fee farm rent in Sussex, £19 4*s*. per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.

£14,000 Reduced 3 per cent. Annuities.

£25,169 16*s*. Consolidated Bank Annuities.

£513 9*s*. 8*d*. New $2\frac{1}{2}$ per cent. Stock.

EDWARD SABINE,

Treasurer.

	£	s.	d.
Balance due to the Treasurer	255	9	8
Bishop of Oxford, Fairchild Lecture	2	16	0
Professor Thomson, Bakerian Lecture	4	0	0
Salaries and Wages	752	10	6
Purchase of £589 11*s*. 9*d*. 3 per cent. Consols	560	0	0
Fire Insurance	45	1	6
Printing Transactions	318	0	3
Ditto Proceedings	143	17	7
Ditto Miscellaneous	56	1	6
Engraving	363	11	8
Paper for Transactions and Proceedings	229	3	0
Binding Transactions	68	17	0
Books Purchased and Binding	246	11	0
Stationery	14	12	0
Shipping Expenses	3	7	10
Fire and Lighting	33	3	0
House Expenses	120	15	0
Taxes	57	5	5
Rumford Fund	133	6	4
Copley Fund	5	11	0
Donation Fund	115	0	0
Wintringham Fund	33	15	0
Postage, Miscellaneous and Petty Charges	80	13	7
Balance in the hands of the Treasurer	136	14	3
	£3780	3	1

Professor Thomson desires to make the following corrections:—

Vol. VII. Page 394, line 6 from foot, *dele* half.
— — 398, — 4 from top, *for* 2,000,000 *read* 5200×10^6; and *for* 8,200,000 *read* 21000×10^6.
— — *ib.* — 10 from top, *for* 10 tons *read* 53600×10^6 tons; and *for* 42 tons *read* 874000×10^6 tons.

Vol. VIII. — 125, — 11 from top, *for* $\sin\theta$ *read* $\cos\theta$.
— — 128, — 3 from foot, *for* $\phi_2 - (\xi)$ *read* $-\phi_2(\xi)$.
— — *ib.* — 2 from foot, *for* $\psi_1(\theta)+\psi_2(\theta)$ *read* $\psi_1(\theta)-\psi_2(\theta)$.
— — 157, — 10 from top, *for* no *read* or without.
— — *ib.* — 11 from foot, *dele* Hence.
— — *ib.* — 8 and 7 from foot, *for* at the rate of one turn in $8\frac{n^4}{\lambda^4}\frac{a^3}{s^3}$ wave lengths *read* at a certain rate.
— — 181, — 13, *for* the part *read* the narrow part.
— — *ib.* — 16, *for* a *read* $\frac{Q}{a}$.
— — 182, — 2, *for* 268° *read* 238°.
— — *ib.* — 3, *for* 19° *read* 49°.

December 11, 1856.

General SABINE, R.A., V.P. and Treasurer, in the Chair.

The Chairman announced that the President had appointed the following gentlemen Vice-Presidents :—

Major-General Sabine.
The Very Rev. The Dean of Ely.
William Robert Grove, Esq.
William Allen Miller, M.D.
Rear-Admiral Sir James Clark Ross.
Rear-Admiral William Henry Smyth.

The following communications were read :—

I. "Observations made to ascertain the Specific Gravity of Sea-water in the Northern and Southern Hemispheres." By Rear-Admiral PHILIP P. KING, R.N., F.R.S. &c. Received October 15, 1856.

(Abstract.)

The specimens of sea-water experimented upon were collected during the voyage of Her Majesty's Ship 'Adventure,' commencing at Rio de Janeiro, and from thence in succession to St. Catharine's, the River Plate, round the Falkland Islands to Cape Horn, and thence to Valparaiso ; and during the ship's return from Valparaiso, through the Strait of Magalhaens to the River Plate and Rio de Janeiro. The series was then completed by the voyage to England.

From the author's observations it may be inferred that the density of the water of the Ocean is, very nearly, identical in all parts of the Atlantic between 40° North and 40° South latitude, the exceptions being due to local causes. Dry winds, by increasing the effect of evaporation, would naturally increase the density of the surface water,

whilst on the other hand, winds charged with vapour would have but little effect; and a heavy fall of rain, particularly in equatorial parts of the Ocean, where the sea is so little disturbed, would very sensibly diminish it. It is also very sensibly less in the vicinity of the coast, particularly when the latter is of a shoal character, as is the case between the River Plate and the Strait of Magalhaens, where the whole extent is fronted by a bank having from 30 to 50 fathoms of water.

The mean specific gravity of the water of the South Pacific, contained between the parallels of 10° and 40°, is 1026·48, and between 40° and 60° it is 1026·13.

The results obtained by the author are then compared with the following, viz.—

Observations made on board the 'Hamlet' in 1849, during a voyage from Sydney to England.

Observations made on board the 'Thomas Arbuthnot' in 1849 and 1850, during a voyage from England to Sydney.

Specific gravities of specimens of water collected by Captain J. Elphinstone Erskine, R.N., of Her Majesty's Ship 'Havannah,' the Senior Officer on the Australian Station, during a visit to New Caledonia, the Loyalty Islands, and to those at the south-eastern end of the Solomon Islands.

Specific gravities of specimens of water collected by the late Captain Sir James Everard Home, Bart., R.N., C.B., of Her Majesty's Ship 'Calliope,' the successor of Captain Erskine in the command of the Station, who visited the Friendly and the Fidjee Islands.

Specific gravities of specimens of water collected by Mr. Simpson, who commanded a trading vessel between China and Sydney, from the Indian Ocean between the Strait of Sunda and the Latitude of 36° South.

II. "On the Existence of Silver in Sea-water." By FREDERICK FIELD, F.C.S. Communicated by MICHAEL FARADAY, Esq., D.C.L., F.R.S. &c. Received October 23, 1856.

In a paper first published by MM. Malaguti, Durocher, and Sarzeaud in the 'Annales de Chimie et de Physique,' xxviii. p. 129,

and translated in the Quarterly Journal of the Chem. Soc., vol. iii. p. 69, there is an account of the detection and estimation of silver in sea-water. The authors suspected the existence of the metal from the extensive diffusion of silver in the mineral kingdom, the conversion of its sulphide into chloride by the prolonged action of soluble bodies containing chlorine, and the solubility of chloride of silver in chloride of sodium. The method pursued was by passing sulphuretted hydrogen through large quantities of water, and also by fusing the salts obtained by evaporation with litharge and subsequent cupellation.

As a solution of chloride of silver in chloride of sodium is instantly decomposed by metallic copper, chloride of copper being formed and silver precipitated, it appeared to me highly probable that the copper and the yellow metal used in sheathing the hulls of vessels, must, after long exposure to sea-water, contain more silver than they did before having been exposed to its action, by decomposing chloride of silver in their passage through the sea, and depositing the metal on their surfaces. A large vessel, the 'Ana Guimaraens,' now under the Chilian flag, was hauled down in the Bay of Herradura, near Coquimbo, for the purpose of being repaired, and the captain obligingly furnished me with a few ounces of the yellow metal from the bottom of the vessel. The investigation was interesting, as the metal had been on for more than seven years (an unusually long period), and the ship had been trading up and down the Pacific Ocean all that time. The metal, upon examination, was found to be exceedingly brittle, and could be broken between the fingers with great ease. 5000 grs. were dissolved in pure nitric acid and the solution diluted; a few drops of hydrochloric acid were added, and the precipitate allowed to subside for three days. A large quantity of white insoluble matter had collected by that time at the bottom of the beaker. This was filtered off, dried, and fused with 100 grs. pure litharge and suitable proportions of bitartrate of potash and carbonate of soda, the ashes of the filter also being added. The resulting button of lead was subsequently cupelled, and yielded 2·01 grs. silver, or 1 lb. 1 oz. 2 dwts. 15 grs. troy per ton. This very large quantity could hardly be supposed to have existed in the original metal, as the value of the silver would be well worth the extraction. It is to be regretted that the captain had

none of the original on board. A piece of yellow metal with which he was repairing the vessel yielded only 0 oz. 18 dwts. to the ton. I was enabled by the courtesy of the captain of the 'Nina,' a brig which had just arrived in the Pacific from England, to obtain more satisfactory information. He gave me a piece of Muntz's yellow metal from his cabin, from the same lot with which the brig was sheathed, but which had never been in contact with salt water; and also a small portion from the hull of the ship after it had been on nearly three years. The experiments were performed as before, and the results were very striking.

grs.		gr.		oz.	dwts.	grs.	
1700	from cabin gave	·051=·003	per cent.=	0	19	14	per ton.
1700	from hull ,,	·400=·023	,, =	7	13	1	,, ,,

That which had been exposed to the sea having nearly eight times as much silver as the original sample.

Many other specimens were examined of metals from the bottoms of ships, and of pieces which are always kept on board in case of need, and it was invariably found that the former contained more silver than the latter. For instance, a piece from the hull of the 'Bergmann' gave 5 ozs. 16 dwts. 18 grs. per ton, while that from the cabin yielded 4 ozs. 6 dwts. 12 grs. 200 grs. from a piece from the hull of the 'Parga' gave ·072 gr., and a piece of fresh metal ·050; while from the 'Grasmere,' only coppered a few months, 610 grs. from the hull gave ·075 and from the cabin ·072,—a very slight difference indeed.

It will be observed that the amount of silver in the above specimens of fresh metal is very high, and it is probable that most of these are merely the re-rolling of masses of metal melted down from old sheathing, and have derived the greater part of their silver from the sea on former occasions. It is well known that the copper used in the manufacture of yellow metal is very pure, containing 2 or 3 dwts. of silver per ton, frequently not so much, and silver is very seldom associated with the other constituent, zinc. In order to arrive at more certain results, however, I have granulated some very pure copper, reserving some in a glass stoppered bottle, and suspended the remainder (about 10 ounces) in a wooden box perforated on all sides, a few feet under the surface of the Pacific Ocean. When occasion

offers, the box is towed by a line at the stern of a vessel which is trading up and down the coast of Chili. It is almost too soon to expect any decisive results at present, but in a few months I hope to be enabled to send both the original copper, and that which has been exposed to the action of the sea.

III. "On the Causes of the great Variation among the different Measures of the Earth's Mean Density." By Captain W. S. Jacob, Director of the Observatory at Madras. Communicated by the Rev. Baden Powell, F.R.S. Received October 25, 1856.

The result of the Pendulum experiments in the Harton Colliery, undertaken by the Astronomer Royal in 1854, and detailed in his paper presented to the Royal Society in January 1856, appears at first sight rather startling, as adding to the already somewhat discrepant measures or estimates of the earth's mean density one more discordant than ever; so that we have now values ranging from 4·7 to nearly 6·6; a range, which, in the absence of any sufficient ground for selecting any *one* as true to the exclusion of the rest, would seem to deprive us of all confidence in their correctness as *measures*, and leave them rather to be classed as *estimates* of a very rough description.

But it will be my endeavour to show, that, while none of the methods employed are capable of giving *strictly* accurate results, the Cavendish experiment is the one which may be relied on as giving a good approximation to the truth, within limits of error (when conducted with proper precaution) far less than those to which either of the other methods are liable.

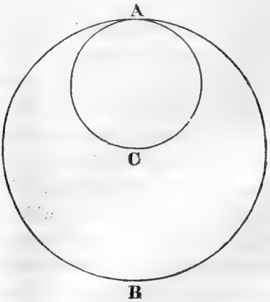
The three principal methods which have been tried are,—1st, the *Schehallien* or *Huttonian*, which consists in comparing the total attraction of the earth with that of a mountain mass, by measuring astronomically the inclination of the normals at a given distance in the meridian-plane on each side of the mass; and then inferring the attraction of the mass from the difference of this inclination from what it would be on an exact spheroid; 2nd, the *Cavendish* experi-

ment, in which the earth's total attraction is compared, by means of the torsion-balance, with that of a small mass of known dense material; and 3rd, the *pendulum*, or *Airy's* experiment, in which the total attraction is compared with that at some distance below the surface, or by means of *differences*, with that of the outer spheroidal shell, whose density may be supposed, approximately at least, to be known.

Now none of these methods give the mean density as a direct result; for the result obtained, the earth's total attraction, is $= g \times$ the sum of (all the particles divided respectively by the squares of their distances) instead of $g \times$ (the total mass divided by the square of the radius or mean distance): and to assume the equality of these, is to assume the earth to be a sphere, and to have its matter arranged in concentric shells or layers of equal density throughout each layer, both of which we know to be untrue. Mr. Airy has indeed shown that, in the case of his experiment, it is sufficient if we know, as regards the upper shell, the form and density of that portion which is in the immediate neighbourhood of the place of observation, without attending to irregularities of distant parts; but he has *not* shown that variations of density *below* and *near* to his lowest station would not sensibly vitiate his results.

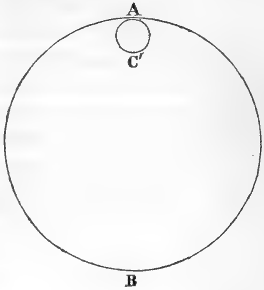
In order to show the nature and amount of error that might thus be introduced, let AB be a section of the earth through the centre, AC an inscribed sphere of half the diameter; then it is evident that on the supposition of a uniform density throughout, the attraction of the small sphere on the point A would be just half of the total attraction of the earth, although its mass would amount to but $\frac{1}{8}$; and if this small sphere were to have its density doubled, the total attraction at A would be increased by one-half, while the *mean* density would be altered by only $\frac{1}{8}$.

Fig. 1.



Now it is quite true that we need not fear any great deviation of density extending through so large a portion of the earth, for that would displace the centre of gravity to a sensible extent, which would become perceptible in the measurements of latitude; but a local deviation might produce a *smaller* but yet *sensible* amount of error; thus if AC′ (fig. 2) represent a sphere of 100 miles diameter, the attraction of this on the point A will $= \frac{1}{80} \frac{dg}{D}$; where D is the earth's mean density, d that of the small sphere, and g the total attraction at A. If, therefore, d should be changed to d', the attraction at A, or the *apparent mean density*, will be altered in the proportion of $1 : 1 + \frac{d'-d}{80d}$; which might be a sensible quantity without producing any sensible effect on the *true* mean density, or on the position of the centre of gravity, since the bulk of AC′ would be only about $\frac{1}{500,000}$ of that of the earth. Now we know little or nothing of the density of the matter a few miles below the surface; only we are sure, from the discordant lengths of the pendulum in different latitudes, and even in the same latitude under different meridians, that the local deviations are indeed *sensible*, yet of so small an amount as hardly to affect this inquiry, and that the error from this cause can never even approach 1 per mille. The Cavendish experiment may therefore be considered as practically free from error of this kind; and as regards errors arising from manipulation or instrumental causes, their probable amount may be determined in the ordinary way from the variation of the results. But if the *Cavendish*, why may not the *Huttonian* experiment equally be considered free from error? Because in the former we are dealing with disturbing masses whose amount is exactly known, whereas in the latter, while we may approximately measure the mass of the mountain *above* the surface, we do not know how much may be added or abstracted

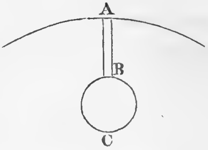
Fig. 2.



below; and we have no right to assume that the mountain is merely a detached mass resting upon the general surface; it will almost certainly have *roots* differing in density from the surrounding country, as has been ably shown by Mr. Airy in the Philosophical Transactions for 1855, page 101.

In the case of the pendulum experiment the uncertainty is somewhat greater; thus, let AB represent the Harton pit, and let BC be a sphere of lead (supposed to lie at its foot) of a diameter equal to AB (*i. e.* about $\frac{1}{4}$ mile); the density of this being about double the mean density of the earth and about quadruple that of the neighbouring country, its excess of attraction on B will be $=\frac{g}{21000}$, while its attraction at A will be only $\frac{1}{9}$ of this, and the difference of its attraction on the two stations will therefore be $\frac{g}{23625}$, being only a little less than the whole quantity observed by Mr. Airy. We may indeed be pretty sure that there is no such mass of *lead*, or mineral of nearly equal density, at the foot of the Harton shaft, yet it is quite conceivable that there should be, within the sphere BC, an excess of density amounting to $\frac{1}{8}$ of that of lead, or about 1·4; and this would produce a difference of effect on A and B amounting to $\frac{g}{144,000}$, and would alter the value of $\frac{D}{d}$* to 2·384, and that of E† to 5·96, approaching considerably nearer to Baily's determination. Further, there must doubtless be a small latitude allowed to the assumed density of the upper strata, the average of which, within the limits that would affect the pendulum, may not be exactly the same as in the immediate vicinity of the pit; supposing it to be 2·4 instead of 2·5, E will be reduced to 5·72, being nearly identical with Baily's value.

Fig. 3.



If it should be objected that so large a variation of density as that assumed above (1·4), though *possible*, is not likely; the same effect

* Ratio of mean density of the earth to that at the surface.
† Earth's mean density.

might be produced by a smaller rate of change through a greater space; thus an addition of about 0·5 to the specific gravity of a sphere of 1 mile in diameter, or of 0·33 to one of 10 miles diameter, would have nearly the same effect, and it cannot be contended that these are improbably large.

Should the experiment ever be repeated, it would be desirable to swing the pendulum at one (at least) intermediate station between the top and bottom of the shaft, by which means any error of this kind might be approximately eliminated. In the mean time I think there are hardly sufficient grounds for impugning the correctness of the value of E (5·67) deduced by the late Francis Baily from his carefully conducted repetition of the Cavendish experiment.

IV. "On Practical Methods for rapid Signalling by the Electric Telegraph." By Prof. W. Thomson, F.R.S. Received November 14, 1856.

I am at present engaged in working out various practical applications of the formulæ communicated some time ago in a short article on the "Theory of the Electric Telegraph" (Proceedings, May 17, 1855), and I hope to be able very soon to lay the results in full before the Royal Society. In the mean time, as the project of an Atlantic Telegraph is at this moment exciting much interest, I shall explain shortly a telegraphic system to which, in the course of this investigation, I have been led, as likely to give nearly the same rapidity of utterance by a submarine one-wire cable of ordinary lateral dimensions between Ireland and Newfoundland, as is attained on short air or submarine lines by telegraphic systems in actual use.

Every system of working the electric telegraph must comprehend (1) a plan of operating at one extremity, (2) a plan of observing at the other, and (3) a code of letter-signals. These three parts of the system which I propose will be explained in order,—I. for long submarine lines, and II. for air or short submarine lines.

I. *Proposed telegraphic system for long submarine lines.*

1. *Plan of operating.*—This consists in applying a regulated gal-

vanic battery to give, during a limited time, a definite variation of electric potential determined by theory, so as to fulfil the condition of producing an electric effect at the other extremity, which, after first becoming sensible, rises very rapidly to a maximum, then sinks as rapidly till it becomes again, and continues, insensible.

The principle followed is that pointed out by Fourier, by which we see, that, when the wire is left with both ends uninsulated after any electrical operations whatever have been performed upon it, the distribution of electric potential through it will very quickly be reduced to a harmonic law, with an amplitude falling in equal proportions during equal intervals of time. Unless the electric operations fulfil a certain condition, this ulterior distribution is according to the simple harmonic law (that is, is proportional to the sine of the distance from either extremity, the whole length being reckoned as 180°). The condition which I propose to fulfil is, that the coefficient of the simple harmonic term in the expression for the electrical potential shall vanish. Then, according to Fourier, the distribution will very much more quickly wear into one following a double harmonic law (that is, the sine of the distance from one extremity, the whole length being reckoned as 360°). In this state of electrification the two halves of the wire on each side of its middle point, being symmetrically and oppositely electrified, will discharge into one another, as well as into the earth at their remote extremities; each will be like a single wire of half-length, with the simple harmonic distribution; and the wire will, on the whole, be discharged as fast as a wire of half the length, or four times as fast as a wire of the whole length, after an ordinary electrification. There is considerable latitude as to the mode of operating so as to fulfil this condition, but the theoretical investigation is readily available for finding the best way of fulfilling it in practice. The result, as I have tested by actual calculation of the electric pulse at the remote end, is most satisfactory. The calculations, and curves exhibiting the electric pulse in a variety of cases, will, I trust, very soon be laid before the Royal Society.

The time and law of operations being once fixed upon, a mechanical contrivance of the simplest kind will give the means of directing a regulated galvanic battery to perform it with exactness, and to any stated degree (positive or negative) of strength. Complete

plans of all details I have ready to describe when wanted, and shall very soon be able to state exactly the battery power required for a cable of stated dimensions.

2. *Plan of observation for receiving a message.*—The instrument which I propose is Helmholtz's galvanometer, with or without modification. The time of vibration of the suspended magnet, and the efficiency of the copper damper, will be so arranged, that during the electric pulse the suspended magnet will turn from its position of equilibrium into a position of maximum deflection, and will fall back to rest in its position of equilibrium. The possibility of fulfilling these conditions is obvious from the form of the curve I have found to represent the electric pulse. The observer will watch through a telescope the image of a scale reflected from the polished side of the magnet, or from a small mirror carried by the magnet, and he will note the letter or number which each maximum deflection brings into the middle of his field of view.

3. *Code of letter-signals.*—The most obvious way of completing a telegraphic system on the plans which have been described, is to have the twenty-six letters of the alphabet written on the scale of which the image in the suspended mirror is observed, and to arrange thirteen positive and thirteen negative strengths of electric operation, which will give deflections, positive or negative, bringing one or other of these letters on the reflected scale into the centre of the field of view. But it would be bad economy to give the simple signals to rare letters, and to require double or triple signals for double and triple combinations of frequent occurrence. Besides, by the plans which I have formed, it will, I believe, be easy to make much more than thirteen different positive and thirteen different negative strengths of electric operation, giving unmistakeably different degrees of deflection; and if so, then many of the most frequent double and triple combinations, as well as all the twenty-six letters of the alphabet singly, might be made by simple signals. But it is also possible (although I believe highly improbable), that in practice only three or four, or some number less than thirteen, of unmistakeably different deflections could be produced in the galvanometer at one end by electric operations performed on the other extremity. If so, the whole twenty-six letters could not each have a simple signal, and double signals would have to be chosen for the less frequent letters.

Experience must show what number of perfectly distinct simple signals can be made, and I have scarcely a doubt but that it will be much more than twenty-six. Then it will be easy to invent a letter code which will use these signals with the best economy for the language in which the message is to be delivered. Towards this object I have commenced collecting statistics showing the relative frequency with which the different simple letters, and various combinations of simple letters, occur in the English language, and I must soon have information enough to guide in choosing the best code for a given number of simple signals.

The investigation leading to a measurement of the electro-magnetic unit of electricity in terms of the electro-static unit, published since the commencement of the present year by Kohlrausch and Weber, has given all that is required to deduce from Weber's own previous experiments the measurement of the electric conductivity of copper wire in terms of the proper kind of unit for the telegraph problem. The data required for estimating the rapidity of action in a submarine wire of stated dimensions would be completed by a determination of the specific inductive capacity of gutta percha, or better still, a direct experiment on the electro-static capacity of a yard or two cut from the cable itself. I have estimated the retardations of various electric pulses, and the practicable rate of transmitting messages by cables 2400 miles long, and of certain ordinary lateral dimensions, on the assumption that the specific inductive capacity of gutta-percha, measured as Faraday did that of sulphur, shell-lac, &c., is 2, from which it probably does not differ much. These estimates have been published elsewhere (Athenæum, Oct. 1856), and I shall not repeat them until I can along with them give a table of estimates for cables of various dimensions, with the uncertainty as to the physical property of gutta-percha either done away with by experiment, or taken strictly into account.

II. *Plan for rapid self-recording signals by air wires and short submarine cables.*

The consideration of the preceding plans has led me to think of a system of working air lines, and short submarine lines, by which great rapidity of utterance, considerably greater I believe than any hitherto practised, may be attained. I have no doubt but that on

this system five or six distinct letters per second, or sixty words per minute, may be readily delivered through air lines and submarine lines up to 100 miles, or perhaps even considerably more, of length, and recorded by a self-acting apparatus, which I shall describe in a communication I hope to make to the Royal Society before its next meeting.

V. "On Practical Methods for Rapid Signalling by the Electric Telegraph." (Second communication.) By Prof. W. Thomson, F.R.S. Received December 11, 1856.

I. *Further remarks on proposed method for great distances.*

Since my former communication on this subject I have worked out the determination of operations performed at one extremity of a submarine wire, so adjusted, that when the other extremity is kept constantly uninsulated, the subsidence of the electricity in the wire shall follow the *triple harmonic law* (that is to say, the electrical potential shall ultimately vary along the wire in proportion to the sine of the distance from either end, one-third of the length of the wire being taken as 180°). The condensation of the electrical pulse at the receiving extremity, due to such operations, is of course considerably greater than that which is obtained from operations leading only to *the double harmonic* as described in my last communication; but experience will be necessary to test whether or not the precision of adjustment in the operations required to obtain the advantages which the theory indicates, can be attained in practice when so high a degree of condensation is aimed at. The theory shows exactly what amount and duration of residual charge in the wire would result from stated deviations from perfect accuracy in the adjustments of the operations; but it cannot be known for certain, without actual trial, within what limit such deviations can be kept in practice. From Weber's experiments on the electric conductivity of copper, and from measurements which I have made on specimens of the cable now in process of manufacture for the Atlantic telegraph, I think it highly probable that, with an alphabet of twenty letters, one letter could be delivered every two seconds between Newfoundland and Ireland (which would give, without any condensed code, six

words per minute) on the general plan which I explained in my last communication; and that no higher battery power than from 150 to 200 small cells of Daniell's (perhaps even considerably less) would be required. Whether or not this system may ultimately be found preferable to the very simple and undoubtedly practicable method of telegraphing invented by Mr. Wildman Whitehouse, can scarcely be decided until one or both methods shall have been tested on a cable of the dimensions of the Atlantic cable, either actually submerged or placed in perfectly similar inductive circumstances.

II. *Method for telegraphing through submarine or subterranean lines of not more than* 500 *miles length.*

The plan which I have proposed to describe for rapid signalling through shorter wires, has one characteristic in common with the plan I have already suggested for the Atlantic telegraph; namely, that of using different strengths of current for different signals.

But in lines of less than 500 miles, condensed pulses, such as have been described, may be made to follow one another more rapidly than to admit of being read off by an observer watching the image of a scale in a suspended mirror; and a new plan of receiving and recording the indications becomes necessary.

Of various plans which I have considered, the following seems most likely to prove convenient in practice.

Several small steel magnets (perhaps each about half an inch long) are suspended horizontally by fine threads or wires at different positions in the neighbourhood of a coil of which one end is connected with the line wire and the other with the earth. Each of these magnets is held in a position deflected from the magnetic meridian by two stops on which its ends press; and two other small stops of platinum wire are arranged to prevent it from turning through more than a very small angle when actuated by any deflecting force making it leave the first position. When a current passing through the coil produces this effect on any one of these magnets, it immediately strikes the last-mentioned stops, and so completes a circuit through a local battery and makes a mark on prepared electro-chemical paper. For each suspended magnet there is a separate style, but of course one battery is sufficient for the whole

printing process. One set of the different suspended magnets are so adjusted, that a current in one direction of any strength falling short of a certain limit makes only one of them move; that a current in the same direction, of strength exceeding this limit but falling short of another limit, moves another also of the suspended magnets; and so on for a succession of different limits of strength of current in one direction. The remaining set of suspended magnets are adjusted to move with different strengths of current in the other direction through the coil. Without experience it is impossible to say how many gradations of strength could be conveniently arranged to be thus distinguished unmistakeably. I have no doubt, however, that very moderate applications of electric resources would give at least three different strengths of current in each direction, which could with ease and certainty be distinguished from one another by the test which the suspended magnets afford. Thus, a signal of six varieties —one letter of an alphabet of six—could be recorded by almost instantaneous movements of six suspended magnets, making one, two or three marks by one set of three styles, or one, two or three marks by another set of three styles, placed all six beside one another, pressing on a slip of electro-chemical paper drawn by clockwork, as in the Morse instrument.

In subterranean or submarine lines of less than 100 miles length, it would be easy, by means of simple battery applications, followed by connexions with the earth, or by means of simple electro-magnetic impulses at one end of the wire, to give ten or twelve of such signals per second without any confusion of utterance at the other end. The confusion of utterance which would be experienced in working thus through longer lines would be easily done away with, in any length up to 500 miles, by following up each battery application with a reverse application for a shorter time, or by following up each electro-magnetic impulse by a weaker reverse impulse, so as approximately to fulfil the condition (described in my former communication), of reducing the subsidence of the electrification in the wire to the double harmonic form. It would, I believe, be readily practicable to send distinctly five or six such signals per second (each a distinct letter of an alphabet of six) through a wire of 500 miles length in a submarine cable of ordinary dimensions. To perform the electrical operations required for sending a message on this system, mechanism might be

had recourse to, and, by the use of perforated slips, as in Bain's and other systems, it would be easy to work from twelve to twenty of the six-fold varied signals per second through lines of less than 100 miles length. Operating by the hand is, however, I believe, generally preferred for ordinary telegraphing; and no such speed as the last-mentioned could be attained even by a skilful operator working with both hands. Six distinct letters or signs of an alphabet of thirty, could, however, I believe, be delivered per second by the two hands working on a key-board with twelve keys (perhaps like those of a pianoforte), provided the keys are so arranged as to fulfil the following conditions:—

(1) That by simply striking once any one of a first set of six of the keys, an electric operation of one or other of the six varieties shall be made twice, the second time commencing at a definite interval (perhaps $\frac{1}{12}$th of a second) later than the first.

(2) That by striking one or other of the remaining six keys at the same time, or very nearly at the same time, as one of the first set, the second operation of the double electric signal will be that corresponding to the key of the second set which is struck, instead of being a mere repetition of the operation corresponding to the key of the first set.

It would certainly be easy to make a key-board to fulfil these conditions with the aid of some clockwork power. Then by arranging the thirty-six permutations and doubles of the six simple signals to represent an alphabet of thirty-six letters and signs, an experienced operator would have to direct his mind to only six different letters per second, while executing them by six double operations with his fingers. That it would be possible to work by hand at this rate there can be no doubt, when we consider the marvels of rapid execution so commonly attained by practice on the pianoforte; and it appears not improbable that in regular telegraphic work, practised operators of ordinary skill could perform from four to six letters with ease per second, or from forty to sixty words per minute, on lines of not more than 100 miles length. The six signals per second, which, according to the preceding estimate, could be distinctly conveyed by a submerged wire of 500 miles in length, could of course be easily performed by the hand, with the aid of a key-board and clockwork power adapted to make the double operations for giving rapid sub-

sidence of electricity in the wire when any one key is touched, and to let the different strengths of current, in one direction or the other, be produced by the different keys. Thus without a condensed code, thirty words per minute could be telegraphed through subterranean or submarine lines of 500 miles; and from thirty to fifty or sixty words per minute through such lines, of lengths of from 500 miles to 100 miles.

The rate of from fifty to sixty words per minute could be attained through almost any length of air line, were it not for the defects of insulation to which such lines are exposed. If the imperfection of the insulation remained constant, or only varied slowly from day to day with the humidity of the atmosphere, the method I have indicated might probably, with suitable adjustments, be made successful; and I think it possible that it may be found to answer for air lines of hundreds of miles' length. But in a short air line, the strengths of the currents received, at one extremity, from graduated operations performed at the other, might suddenly, in the middle of a message, become so much changed as to throw all the indications into confusion, in consequence of a shower of rain, or a trickling of water along a spider's web.

VI. "On the Equation of Laplace's Functions," &c. By W. F. Donkin, M.A., F.R.S., F.R.A.S., Savilian Professor of Astronomy, Oxford. Received December 3, 1856.

(Abstract.)

The equation $\frac{d^2u}{dx^2}+\frac{d^2u}{dy^2}+\frac{d^2u}{dz^2}=0$, when transformed by putting $x=r\sin\theta\cos\phi$, $y=r\sin\theta\sin\phi$, $z=r\cos\theta$, may be written in the form

$$\left\{\left(\sin\theta\frac{d}{d\theta}\right)^2+\left(\frac{d}{d\phi}\right)^2+(\sin\theta)^2r\frac{d}{dr}\left(r\frac{d}{dr}+1\right)\right\}u=0; \quad (1)$$

and if $u=u_0+u_1r+u_2r^2+\ldots+u_nr^n+\ldots$, we find on substituting this value in (1), and equating to zero the coefficient of r^n, that u_n satisfies the equation

$$\left\{\left(\sin\theta\frac{d}{d\theta}\right)^2+\left(\frac{d}{d\phi}\right)^2+n(n+1)(\sin\theta)^2\right\}u_n=0, \quad . \quad . \quad (2)$$

commonly called the equation of Laplace's functions. If we put $\sin\theta\frac{d}{d\theta}+n\cos\theta=\varpi_n$, then the equation (2) may be written

$$\left(\varpi_n\varpi_{-n}+n^2+\left(\frac{d}{d\phi}\right)^2\right)u_n=0\,;$$

and the operation ϖ_n possesses the following property, namely

$$\varpi_{-n}\varpi_n+n^2=\varpi_{n-1}\varpi_{-(n-1)}+(n-1)^2\,;$$

hence it is easily shown, that in general the complete solution of (2) is

$$u_n=\varpi_n\varpi_{n-1}\ldots\varpi_2\varpi_1\,.\,u_0,$$

where u_0 is the solution of

$$\left(\left(\sin\theta\frac{d}{d\theta}\right)^2+\left(\frac{d}{d\phi}\right)^2\right)u_0=0,$$

namely

$$u_0=f\left(e^{\phi\sqrt{-1}}\tan\frac{\theta}{2}\right)+\mathrm{F}\left(e^{-\phi\sqrt{-1}}\tan\frac{\theta}{2}\right)\,;$$

and the operation $\varpi_n\varpi_{n-1}\ldots\varpi_2\varpi_1$ is easily seen to be equivalent to

$$(\sin\theta)^{-n}\left(\sin\theta\frac{d}{d\theta}\sin\theta\right)^n.$$

(This result is compared with that obtained in a different way by Professor Boole (Cambridge and Dublin Journal, vol. i. p. 18), to which it bears a general resemblance, but the author has not succeeded at present in reducing the one form to the other.)

In the case in which u_n does not contain ϕ, we have

$$u_0=\mathrm{C}_1+\mathrm{C}_2\log\tan\frac{\theta}{2}.$$

The general expression for a "Laplace's coefficient" of the nth order, not containing ϕ, is therefore $(\sin\theta)^{-n}\left(\sin\theta\frac{d}{d\theta}\sin\theta\right)^n.\mathrm{C}$; and if this be called v_n when $\mathrm{C}=1$, the development of $(1-2r\cos\theta+r^2)^{-\frac{1}{2}}$ is

$$v_0+v_1r+v_2\frac{r^2}{1\,.\,2}+\ldots+v_n\frac{r^n}{1\,.\,2\ldots n}+\ldots\,;$$

and it is shown that the coefficient of $\frac{r^n}{1\,.\,2\ldots n}$ in the development of $(1-2r\cos\theta+r^2)^{-\frac{i+1}{2}}$ is

$$(\sin\theta)^{-n-i}\left(\sin\theta\frac{d}{d\theta}\sin\theta\right)^n(\sin\theta)^i.$$

With respect to the development of

$$\left(1-2r(\cos\theta\cos\theta'+\sin\theta\sin\theta'\cos\phi)+r^2\right)^{-\frac{1}{2}},$$

it is shown that the coefficient of $r^n \cos i\phi$ may be put in either of the two forms,

$$\frac{2}{1\,.\,2\,\ldots\,(n-i)\,.\,1\,.\,2\,\ldots\,(n+i)}(\sin\theta)^{-n}(\sin\theta')^{-n}\Theta^n\Theta'\left(\tan\frac{\theta}{2}\right)^i\left(\tan\frac{\theta'}{2}\right),$$

or

$$\frac{2\,.\,1^2\,.\,3^2\,\ldots\,(2i-1)^2}{1\,.\,2\,\ldots\,(n-i)\,.\,1\,.\,2\,\ldots\,(n+i)}(\sin\theta)^{-n}(\sin\theta')^{-n}\Theta^{n-i}\Theta'^{\,-i}(\sin\theta)^{2i}(\sin\theta')^{2i},$$

where Θ represents the operation $\sin\theta\dfrac{d}{d\theta}\sin\theta$, and the factor 2 is in each case to be omitted when $i=0$. (This coefficient is a solution of the equation

$$\left\{\left(\sin\theta\frac{d}{d\theta}\right)^2+n(n+1)(\sin\theta)^2-i^2\right\}u=0,$$

of which the complete integral may be expressed in the form

$$(\sin\theta)^{-n}\left(\sin\theta\frac{d}{d\theta}\sin\theta\right)^{n-i}(\sin\theta)^{2i}\left(C_1+C_2\int d\theta(\sin\theta)_{-2i-1}\right),$$

at least in the case in which i is an integer not greater than n, for which case this form is here demonstrated.)

If it be assumed that the solution of (2), obtained on the supposition that n is an integer, may be extended to the case in which n is a general symbol, it follows that the solution of (1) will be obtained from it by changing n into $r\dfrac{d}{dr}$. This would give

$$u=(\sin\theta)^{-r\frac{d}{dr}}\left(\sin\theta\frac{d}{d\theta}\sin\theta\right)^{r\frac{d}{dr}}\left\{f\left(r,\ e^{\phi\sqrt{-1}}\tan\frac{\theta}{2}\right)\right.$$
$$\left.+F\left(r,\ e^{-\phi\sqrt{-1}}\tan\frac{\theta}{2}\right)\right\},$$

which is easily shown to be equivalent to

$$u=f\left(\rho\sin\theta\frac{d}{d\theta}\sin\theta,\ e^{\phi\sqrt{-1}}\tan\frac{\theta}{2}\right)+F\left(\rho\sin\theta\frac{d}{d\theta}\sin\theta,\ e^{-\phi\sqrt{-1}}\tan\frac{\theta}{2}\right),$$

where $\rho=r(\sin\theta)^{-1}$, but ρ is to be treated as a constant till after all operations.

This expression is shown to give known particular integrals, such as $(1-2r\cos\theta+r^2)^{-\frac{1}{2}}$, and

$$r^n(\sin\theta)^{-n}\left(\sin\theta\frac{d}{d\theta}\sin\theta\right)^n\left(\tan\frac{\theta}{2}\right)\cos i\phi.$$

It appears probable, therefore, that the generalization of the result obtained for the limited value of n is legitimate; but the author does not profess to demonstrate this conclusion, believing that the principle of the "permanence of equivalent forms" is not at present established in such a sense as to amount to a demonstration.

VII. "A Memoir on Curves of the Third Order." By Arthur Cayley, Esq., F.R.S. Received Oct. 30, 1856.

(Abstract.)

A curve of the third order, or cubic curve, is the locus represented by an equation such as $U=(*\between x, y, z)^3=0$; and it appears by my "Third Memoir on Quantics," that it is proper to consider, in connexion with the curve of the third order, $U=0$, and its Hessian $HU=0$ (which is also a curve of the third order), two curves of the third class, viz. the curves represented by the equations $PU=0$ and $QU=0$. These equations, I say, represent curves of the third class; in fact, PU and QU are contravariants of U, and therefore, when the variables x, y, z of U are considered as point coordinates, the variables ξ, η, ζ of PU, QU must be considered as line coordinates, and the curves will be curves of the third class. I propose (in analogy with the form of the word Hessian) to call the two curves in question the Pippian and Quippian respectively. A geometrical definition of the Pippian was readily found; the curve is in fact Steiner's curve R_0 mentioned in the memoir "Allgemeine Eigenschaften der algebraischen Curven," Crelle, t. xlvii. pp. 1–6, in the particular case of a basis-curve of the third order; and I also found that the Pippian might be considered as occurring implicitly in my "Mémoire sur les Courbes du Troisième Ordre," Liouville, t. ix. p. 285, and "Nouvelles Remarques sur les Courbes du Troisième Ordre," Liouv. t. x. p. 102. As regards the Quippian, I

have not succeeded in obtaining a satisfactory geometrical definition; but the search after it led to a variety of theorems, relating chiefly to the first-mentioned curve, and the results of the investigation are contained in the present memoir. Some of these results are due to Mr. Salmon, with whom I was in correspondence on the subject. The character of the results makes it difficult to develope them in a systematic order; but the results are given in such connexion one with another, as I have been able to present them in. Considering the object of the memoir to be the establishment of a distinct geometrical theory of the Pippian, the leading results will be found summed up in the nine definitions or modes of generation of the Pippian, given in the concluding number. In the course of the memoir I give some further developments relating to the theory in the memoirs in Liouville above referred to, showing its relation to the Pippian, and the analogy with theorems of Hesse in relation to the Hessian.

VIII. "On the k-partitions of a Polygon and Polyace." By the Rev. T. P. KIRKMAN, M.A. Communicated by ARTHUR CAYLEY, Esq. Received November 13, 1856.

(Abstract.)

The problem relating to the polyace is the reciprocal of that relating to the polygon, and is not separately discussed. By the k-partitions of a polygon, the author means the number of ways in which the polygon can be divided by $(k-1)$ diagonals, no one of which crosses another; two ways being different only when no cyclical permutation or reversion of the numbers at the angles of the polygon can make them alike: it is assumed that the polygon is of the ordinary convex form, so that all the diagonals lie within its area. The author remarks, that the enumeration of the partitions of the polygon and polyace is indispensable in the theory of polyedra, and that in his former memoir "On the Enumeration of x-edra having Triedral Summits and an $(x-1)$-gonal Base," Phil. Trans. 1856, p. 399, he has, in fact, investigated the $(r-2)$-partitions of the r-ace

or r-gon; so that the present memoir may be considered as a completion, or rather an extension and completion, of the investigations in his former memoir. The number of distinctions to be made in the problem of the present memoir is very great; thus, a partition of the polygon may be either reversible or irreversible; and if reversible, then the axis of reversion may be either agonal, monogonal, or diagonal, that is, it may pass through no angle, one angle only, or two angles of the polygon; and in the last case it may be either drawn or undrawn. Again, there may be a single axis or a greater number of axes of reversion: in the case of m such axes, the partition is said to be m-ly reversible; and in like manner an irreversible partition may consist of a single irreversible sequence of configurations, or it may contain such sequence m times repeated; it is then said to be m-ly irreversible. In consequence of this multiplicity of distinctions, the author's final results are necessarily very complicated, and cannot be exhibited in an abstract; they appear, however, to contain a complete solution of the problem, *i. e.* to afford the means of finding, without anything tentative, the number of the k-partitions of an r-gon when k and r are given numbers.

December 18, 1856.

The LORD WROTTESLEY, President, in the Chair.

The following communications were read:—

I. "On the Scelidothere (*Scelidotherium leptocephalum*, Owen), a large extinct Terrestrial Sloth." By Professor R. OWEN, F.R.S. Received October 30, 1856.

(Abstract.)

The extinct species of large terrestrial Sloth, indicated by the above name, was first made known by portions of its fossil skeleton having been discovered by Charles Darwin, Esq., F.R.S., at Punta Alta, Northern Patagonia. These portions were described by the

author in the Appendix to the 'Natural History of the Voyage of H.M.S. Beagle.'

The subsequent acquisition by the British Museum of the collection of Fossil Mammalia brought from Buenos Ayres by M. Bravard, has given further evidence of the generic distinction of the Scelidothere, and has supplied important characters of the osseous system, and especially of the skull, which the fragments from the hard consolidated gravel of Punta Alta did not afford.

The best portion of the cranium from that locality wanted the facial part anterior to the orbit, and the greater part of the upper walls; sufficient however remained to indicate the peculiar character of its slender proportions, and hence Professor Owen has been led to select the name *leptocephalum* for the species, which is undoubtedly new.

The aptness of the epithet 'slender-headed' is proved by the author's researches to be greater than could have been surmised from the original fossil; for the entire skull, now in the British Museum, exhibits a curious and very peculiar prolongation of the upper and lower jaws, and a slenderness of the parts produced anterior to the dental series, unique in the leaf-eating section of the order *Bruta*, and offering a very interesting approximation to the peculiar proportions of the skull in the Ant-eaters.

The original fossils from Patagonia indicated that they belonged to an individual of immature age: the difference of size between them and the corresponding parts in the British Museum, depends on the latter having belonged to full-grown individuals: the slight difference in the shape of the anterior molars seems in like manner to be due to such an amount of change as might take place in the progress of growth of a tooth with a constantly renewable pulp. Professor Owen finds at least no good grounds for inferring a specific distinction between the mature if not old Scelidothere from Buenos Ayres, and the younger specimen from Patagonia.

The author then proceeds to give a detailed anatomical account of the fossil bones in the British Museum, instituting a comparison between them and the bones of other large extinct animals, especially those of the Edentate order.

The Scelidothere was a quadruped of from 8 to 10 feet in length, but not more than 4 feet high, and nearly as broad at the haunches;

the thigh-bones being extraordinarily broad in proportion to their length. The trunk gradually tapered forwards to the long and slender head. The fore-limbs had complete clavicles, and the rotatory movements of the fore-arm. All the limbs were provided with long and strong claws. The animal had a long and muscular tongue, and it is probable that its food might have been of a more mixed nature than in the Megatherium. But it was more essentially related to the Sloths than to the Ant-eaters.

In conclusion the author remarks, that as our knowledge of the great Megatherioid animals increases, the definition of their distinctive characters demands more extended comparison of particulars. Hence in each successive attempt at a restoration of these truly remarkable extinct South American quadrupeds, there results a description of details which might seem prolix and uncalled for, but which are necessary for the proper development of the task of reproducing a specimen of an extinct species.

Professor Owen adds, that he is indebted to an allotment from the Government Grant, placed at the disposal of the Royal Society for scientific purposes, for the means of laying before the Society large and admirably executed drawings of the fossil bones described in his paper.

II. "On the Evidence of the existence of the Decennial Inequality in the Solar-diurnal Variations, and its non-existence in the Lunar-diurnal Variation of the Magnetic Declination at Hobarton." By Major-General Sabine, R.A., D.C.L., Treas. and V.P.R.S. Received Nov. 17, 1856.

(Abstract.)

In a communication made to the Royal Society in the last Session, "On the Lunar-diurnal Magnetic Variation at Toronto," the author had stated that he could discover no trace of the lunar influence of the decennial inequality which constitutes so marked a feature in the solar magnetic variations. He has since read, in a memoir communicated to the Imperial Academy of Sciences at Vienna, entitled "On the Influence of the Moon on the horizontal component of the Mag-

netic Force," that M. Kreil is of opinion that the observations of different years at Milan and Prague, when combined, would rather favour the contrary inference, viz. that the decennial inequality exists in the lunar as well as in the solar variations. The author was led therefore to re-examine this question by the aid of the observations of the Declination at the Hobarton Observatory, which he considers to be remarkably well suited for the purpose, as they comprise eight years of consecutive hourly observation with unchanged instruments and a uniform system of observation, and number, exclusive of Sundays, Christmas-days, and Good Fridays, and occasional but very rare omissions, no less than 51,998 observations.

These observations have been examined by the processes already described in the author's communication of last Session, and the results form the subject of the present paper, showing, in the author's belief, decided and systematic evidence of the existence of the diurnal inequality, having its minimum epoch in 1843–1844, and its maximum epoch five years later, in the mean diurnal variation due to the disturbances and in the more regular and ordinary solar-diurnal variation, and the absence of any trace of a similar inequality in the lunar-diurnal variation.

The Society then adjourned over the Christmas holidays, to January 8, 1857.

January 8, 1857.

WILLIAM ROBERT GROVE, Esq., V.P., in the Chair.

The following communications were read:—

I. "On the Function of the Thyroid Body." By PETER MARTYN, Esq., M.D. Lond., Surgeon R.N. Communicated by Professor HENFREY, F.R.S. Received November 13, 1856.

(Abstract.)

After referring to the form, situation, connexions and internal structure of the thyroid body, its large supply of blood and its capa-

bility of sudden alterations of bulk; the author briefly adverts to the unsatisfactory explanations which have been offered as to its function, and then proceeds to state his own views, as follows:—

"The upper part of the trachea, the larynx, and the passage of the fauces and mouth constitute the organ of voice; the two former are the essential or voicing part as mechanicians call it, that which produces the tone. The larynx and trachea—taking a share in other functions and being associated by juxtaposition and attachment with contiguous organs—are always pervious and open for respiration; lengthen and shorten, fall and rise with the œsophagus in deglutition, and bend and turn with the universal motions of the head and neck.

"To admit of this great mobility and flexibility, a certain structure is necessary. The larynx is a triangular box enclosing the apparatus of the chordæ vocales; its two cartilaginous sides or alæ, diverging from the front, are not fixed but free at the back, being completed by soft parts: the trachea is composed of a succession of incomplete cartilaginous hoops or rings lying apart, the back and intervals being made up and the tube completed by soft membrane.

"Now the structure of a wind instrument, such as that of the human voice is, requires the very opposite properties. It must be rigid, tense and inflexible. The qualities of the tone will be in exact proportion to these properties. How then is the soft, slack and flexible vocal tube rendered thus rigid, tense and inflexible, and fit to produce pure tone? The muscles of the larynx, the thyro-hyoid and sterno-thyroid, merely raise or lower, or fix it in any position: not lying on, or being parallel to, but diverging from the vocal tube, they cannot effect the object referred to. It appears to me that the thyroid body is provided for this purpose. The act of uttering a tone or of speaking stops the return of the blood from that organ, distends and renders it tense, and from the nature of its attachment round the top of the trachea and on the free sides of the alæ of the larynx, renders them fixed, firm, and tense also. This effect is aided by the aforesaid muscles, the thyroid body being interposed and giving them more advantageous mechanical action. This tension may be in any degree, and on energetic speaking or singing, the increased size of the part and the fulness of the collateral veins may be seen. This is the reason of its large supply and free distribution of blood. An instance of the want of this tension in an instrument

may be seen in the bagpipe, where the porte-vent is attached to the chanter or voicing part by a flexible joint or by leather, and the tone is in consequence squeaking and uncertain.

"Besides thus giving rigidity, firmness and tension to the organ of voice, the thyroid body also acts in another capacity—as a loader. In most musical instruments, loaders are used to render the vibrations slower and longer, and the tone in consequence fuller, louder and deeper. They compensate for want of size and space, and give to a small instrument, or to a small vibrating or voicing part of an instrument, the power and quality of a large one. The human organ of voice is 8 inches long, and has the same power and better quality of tone than the instrument that most nearly approaches it,—the French horn, which is 9 feet, or the "vox humana" pipe of a moderate-sized organ, which is from 4 to 8 feet long. This economy of size in the human organ has always been wondered at, but never, that I know, explained. Besides the thyroid body, another part, the structure of which I shall describe on another occasion, aids in this admirable economy. The nearer mechanism of human design approaches to perfection, the more it resembles similar structures in animal mechanics. The base of all stringed instruments and musical boxes is loaded: in most wind instruments the voicing part is thus loaded and strengthened, as in the organ pipe, horn, flute, clarionet, &c. The bassoon, which in its lower notes approaches the human voice, is uncertain and wheezy in tone for want of this provision.

"When the thyroid body is small and thin, the voice will be found to be small and shrill; when large, the tone will be full and sonorous; when it is morbidly enlarged, the voice will be deeper and more base; and when very large, as in bronchocele, the voice will be smothered.

"The compass of the voice is in great part produced by the raising and lowering of the larynx, the shortening and lengthening of the vocal tube. The thyroid body partakes of this motion, at the same time firmly fixing and rendering tense the parts in each position. By its change of shape, bulk and density—flattening and thinning when the larynx is raised, enlarging and bulging when it is lowered—it aids in giving the particular tone or pitch, high and acute in the first case, full and deep in the second; and, in like manner, by its varying shape, bulk, density, and pressure, it takes a great

part in producing the wonderful qualities of modulation and expression peculiar to the human voice. In animated conversation, declamation and singing, this may be seen.

"Its function then appears to be threefold—rendering the slack, mobile and flexible vocal organ or tube rigid, tense and inflexible, and fit to produce pure tone; by its bulk and density acting as a loader and strengthener, making the tone more sonorous, full and deep, and thus compensating for want of length and size in the organ; and finally, by its varying shape, bulk, density and pressure, furnishing an important aid in producing the inimitable qualities of modulation and expression enjoyed by the human voice.

"That it is a part of the organ of voice and an important accessory in giving it perfection, may be inferred also from its situation on the larynx and trachea, and its being supplied by the same nerves—its being largest in man, where the voice and speech are perfect—its being proportionally larger in women and children than in men, their smaller and more mobile organs requiring its peculiar aid. Among the lower animals, it is present (at least in a fully developed condition) only in the Mammalia, but among them there is a remarkable exception in the Cetacea—they have it not, and they have no voice. In Birds, which have such great power and modulation of voice, the structure of the vocal organ and tube is different from that in man, and sufficient in itself to produce these qualities.

"The importance of the thyroid body must be admitted when it is shown to be necessary for the perfection of the voice, and hence of speech—that great and indispensable agent in the cultivating and advancing the highest faculties of man."

II. "Experimental Researches on the Strength of Pillars of Cast Iron." By Eaton Hodgkinson, Esq., F.R.S., Professor of the Mechanical Principles of Engineering, University College, London. Received November 20, 1856.

(Abstract.)

In a previous paper on this subject (Philosophical Transactions, 1840), I had shown,—1st, that a long circular pillar, with its

ends flat, was about three times as strong as a pillar of the same length and diameter with its ends rounded in such a manner that the pressure would pass through the axis, the ends being made to turn easily, but not so small as to be crushed by the weight; 2nd, that if a pillar of the same length and diameter as the preceding had one end rounded and one flat, the strength would be twice as great as that of one with both ends rounded; 3rd, if, therefore, three pillars be taken, differing only in the form of their ends, the first having both ends rounded, the second one end rounded and one flat, and the third both ends flat, the strength of these pillars will be as 1—2—3 nearly.

The preceding properties having been arrived at experimentally, are here attempted to be demonstrated, at least approximately.

The pillars referred to in my former paper were cast from Low Moor iron No. 3; they were very numerous, but usually much smaller than those used in the present trials. I felt desirous too of using the Low Moor iron in the *hollow* pillars employed on this occasion, not on account of its superior strength, but its other good qualities. The pillars from this iron were cast 10 feet long, and from $2\frac{1}{2}$ to 4 inches diameter, approaching in some degree, as to size, to the smaller ones used in practice. The results from the breaking weights of these were moderately consistent with the formulæ in the former paper, with a slight alteration of the constants, rendered necessary by the castings being of a larger size, and therefore softer than before, a matter which will be adverted to further on.

The formulæ for the strength of a hollow pillar of Low Moor iron No. 2,—where w is the breaking weight, in tons, of a pillar whose length is l in feet, and the external and internal diameters D and d in inches, the ends being flat and well bedded—are as below:

$$w = 46{\cdot}65 \times \frac{D^{3{\cdot}55} - d^{3{\cdot}55}}{l^{1{\cdot}7}},$$

from formula in Phil. Trans. 1840;

$$w = 42{\cdot}347 \times \frac{D^{3{\cdot}5} - d^{3{\cdot}5}}{l^{1{\cdot}63}},$$

from formula in present paper.

To obtain some idea of the relative strengths of pillars of different British irons, I applied, at Mr. Stephenson's suggestion, to Messrs.

Easton and Amos, who procured for me twenty-two solid pillars, each 10 feet long and $2\frac{1}{2}$ inches diameter, cast out of eleven kinds of iron (nine simple irons and two mixtures). The pillars were all from the same model, and were cast vertically in dry sand, and turned flat at the ends, as the hollow ones had been; two being cast from the same kind of iron in each case. The simple unmixed irons tried were as below, and all of No. 1.

		Mean breaking weight.
Old Park iron	Stourbridge	29·50 tons.
Derwent iron	Durham	28·03 „
Portland iron	Tovine, Scotland	27·30 „
Calder iron	Lanarkshire	27·09 „
Level iron	Staffordshire	24·67 „
Coltness iron	Edinburgh	23·52 „
Carron iron	Stirlingshire	23·52 „
Blaenavon iron	South Wales	22·05 „
Old Hill iron	Staffordshire	20·05 „

The mean strength of the pillars from the irons above varies from 20·05 to 29·50 tons; or as 2 to 3 nearly.

The pillars formed of mixed irons were found to be weaker than the three strongest of the unmixed series.

From many experiments, it was shown that the weight which would crush the pillars, if they were very short, would vary as 5 to 9 nearly.

The pillars in general were broken of four different lengths, 10 feet, 7 feet 6 inches, 6 feet 3 inches, and 5 feet, the ends of all being turned flat, and perpendicular to the axis. It was found that when the length was the same, the strength varied as the 3·5 power of the diameter; and when the diameter was the same and the length varied, the strength was inversely as the 1·63 power of the length. Both of these were obtained from the mean results of many experiments.

The formula for the strength of a solid pillar would therefore be

$$w = m \times \frac{d^{3\cdot5}}{l^{1\cdot63}},$$

where w is the breaking weight, d the diameter in inches, l the length

in feet, and m a weight which varied from 49·94 tons in the strongest iron we tried, to 33·60 tons in the weakest.

The ultimate decrement of length, in pillars of various lengths but of the same diameter, varies inversely as the length nearly. Thus the ultimate decrements of pillars 10 feet, 7 feet 6 inches, 6 feet 3 inches, and 5 feet, vary as 2, 3, $3\frac{1}{2}$ and 4 nearly, according to the experiments, from which it appeared that the mean decrement of a 10-feet pillar was ·176 inch.

Irregularity in Cast Iron.

The formulæ arrived at in this paper are on the supposition that the iron of which the pillars are composed is uniform throughout the whole section in every part; but this was not strictly the case in any of the solid pillars experimented upon. They were always found to be softer in the centre than in other parts. To ascertain the difference of strength in the sections of the pillars used, small cylinders $\frac{3}{4}$ inch diameter and $1\frac{1}{2}$ inch high, were cut from the centre, and from the part between the centre and the circumference, and there was always found to be a difference in the crushing strength of the metal from the two parts, amounting perhaps to about one-sixth. The thin rings of hollow cylinders resisted in a much higher degree than the iron from solid cylinders. As an example, the central part of a solid cylinder of Low Moor iron No. 2, was crushed with 29·65 tons per square inch, and the part nearer to the circumference required 34·59 tons per square inch; cylinders out of a thin shell half an inch thick, of the same iron, required 39·06 tons per square inch; and other cylinders from still thinner shells of the same metal, required 50 tons per square inch, or upwards, to crush them.

As these variations in cast iron have been little inquired into, except by myself, and have never, so far as I know, been subjected to computation, I have bestowed considerable trouble upon the matter, in an experimental point of view, and endeavoured to introduce into the formulæ previously given, changes which will in some degree include the irregularities observed.

III. "Memoir on the Symmetric Functions of the Roots of certain Systems of two Equations." By ARTHUR CAYLEY, Esq., F.R.S. Received December 18, 1856.

(Abstract.)

The author defines the term *roots* as applied to a system of $n-1$ equations $\phi=0$, $\psi=0$, &c., where ϕ, ψ, &c., are quantics (*i. e.* rational and integral homogeneous functions) of the n variables $(x, y, z, ..)$ and the terms *symmetric functions* and *fundamental symmetric functions* of the roots of such a system; and he explains the process given in Professor Schläfle's memoir, "Ueber die Resultante eines Systemes mehrerer algebraischer Gleichungen," Vienna Transactions, t. iv. (1852), whereby the determination of the symmetric functions of any system of $(n-1)$ equations, and of the resultant of any system of n equations is made to depend upon the very simple question of the determination of the resultant of a system of n equations, all of them, except one, being linear. The object of the memoir is then stated to be the application of the process to two particular cases, viz. to obtaining the expressions for the simplest symmetric functions, after the fundamental ones of the following systems of two ternary equations, viz. first, a linear equation and a quadratic equation; and secondly, a linear equation and a cubic equation; and the author accordingly obtains expressions, as regards the first system, for the fundamental symmetric functions or symmetric functions of the first degree in respect to each set of roots, and for the symmetric functions of the second and third degrees respectively, and as regards the second system, for the fundamental symmetric functions or symmetric functions of the first degree, and for the symmetric functions of the second degree in respect to each set of roots.

IV. "Memoir on the Resultant of a System of two Equations." By ARTHUR CAYLEY, Esq., F.R.S. Received December 18, 1856.

(Abstract.)

The resultant of two equations such as

$$(a, b, ..\between x, y)^m=0$$
$$(p, q, ..\between x, y)^n=0$$

is, it is well known, a function homogeneous in regard to the coefficients of each equation separately, viz. of the degree n in regard to the coefficients $(a, b, \ldots)$ of the first equation, and of the degree m in regard to the coefficients $(p, q, \ldots)$ of the second equation; and it is natural to develope the resultant in the form $k\text{AP}+k'\text{A}'\text{P}'+$ &c., where A, A′, &c. are the combinations (powers and products) of the degree n in the coefficients $(a, b, \ldots)$, P, P′, &c. are the combinations of the degree m in the coefficients $(p, q, \ldots)$, and k, k', &c. are mere numerical coefficients. The object of the present memoir is to show how this may be conveniently effected, either by the method of symmetric functions, or from the known expression of the resultant in the form of a determinant, and to exhibit the developed expressions for the resultant of two equations, the degrees of which do not exceed 4. With respect to the first method, the formula in its best form, or nearly so, is given in the 'Algebra' of Meyer Hirsch, and the application of it is very easy when the necessary tables are calculated: as to this, see my "Memoir on the Symmetric Functions of the Roots of an Equation." But when the expression for the resultant of two equations is to be calculated without the assistance of such tables, it is, I think, by far the most simple process to develope the determinant according to the second of the two methods.

V. "Memoir on the Symmetric Functions of the Roots of an Equation." By Arthur Cayley, Esq., F.R.S. Received December 18, 1856.

(Abstract.)

There are contained in a work, which is not, I think, so generally known as it deserves to be, the 'Algebra' of Meyer Hirsch, some very useful tables of the symmetric functions up to the tenth degree of the roots of an equation of any order. It seems desirable to join to these a set of tables, giving reciprocally the expressions of the powers and products of the coefficients in terms of the symmetric functions of the roots. The present memoir contains the two sets of tables, viz. the new tables distinguished by the letter (a), and the tables of Meyer Hirsch distinguished by the letter (b); the memoir contains

also some remarks as to the mode of calculation of the new tables, and also as to a peculiar symmetry of the numbers in the tables of each set, a symmetry which, so far as I am aware, has not hitherto been observed, and the existence of which appears to constitute an important theorem in the subject. The theorem in question might, I think, be deduced from a very elegant formula of M. Borchardt (referred to in the sequel), which gives the generating function of any symmetric function of the roots, and contains potentially a method for the calculation of the tables (*b*), but which, from the example I have given, would not appear to be a very convenient one for actual calculation.

VI. "Memoir on the Conditions for the Existence of given Systems of Equalities among the Roots of an Equation." By Arthur Cayley, Esq., F.R.S. Received December 18, 1856.

(Abstract.)

It is well known that there is a symmetric function of the roots of an equation, viz. the product of the squares of the differences of the roots, which vanishes when any two roots are put equal to each other, and that consequently such function expressed in terms of the coefficients and equated to zero, gives the condition for the existence of a pair of equal roots. And it was remarked long ago by Professor Sylvester, in some of his earlier papers in the 'Philosophical Magazine,' that the like method could be applied to finding the conditions for the existence of other systems of equalities among the roots, viz. that it was possible to form symmetric functions, each of them a sum of terms containing the product of a certain number of the differences of the roots, and such that the entire function might vanish for the particular system of equalities in question; and that such functions expressed in terms of the coefficients and equated to zero would give the required conditions. The object of the present memoir is to extend this theory, and render it exhaustive by showing how to form a series of types of all the different functions which vanish for one or more systems of equalities among the roots; and in particular to obtain by the method distinctive conditions for all the different

systems of equalities between the roots of a quartic or a quintic equation, viz. for each system conditions which are satisfied for the particular system, and are not satisfied for any other systems, except, of course, the more special systems included in the particular system. The question of finding the conditions for any particular system of equalities is essentially an indeterminate one, for given any set of functions which vanish, a function syzygetically connected with these will also vanish; the discussion of the nature of the syzygetic relations between the different functions which vanish for any particular system of equalities, and of the order of the system composed of the several conditions for the particular system of equalities, does not enter into the plan of the present memoir. I have referred here to the indeterminateness of the question for the sake of the remark that I have availed myself thereof, to express by means of invariants or covariants the different systems of conditions obtained in the sequel of the memoir; the expressions of the different invariants and covariants referred to are given in my "Second Memoir upon Quantics," Phil. Trans. vol. cxlvi. (1856).

VII. "Tables of the Sturmian Functions for Equations of the Second, Third, Fourth and Fifth Degrees." By Arthur Cayley, Esq., F.R.S. Received December 18, 1856.

(Abstract.)

The general expressions for the Sturmian functions in the form of determinants, are at once deducible from the researches of Professor Sylvester in his early papers on the subject in the 'Philosophical Magazine,' and in giving these expressions in the memoir "Nouvelles Recherches sur les Fonctions de M. Sturm," Liouville, t. xiii. p. 269 (1848), I was wrong in claiming for them any novelty. The expressions in the last-mentioned memoir admit of a modification by which their form is rendered somewhat more elegant; I propose, on the present occasion, merely to give this modified form of the general expression, and to give the developed expressions of the functions in question for equations of the degrees, two, three, four and five.

January 15, 1857.

The LORD WROTTESLEY, President, in the Chair.

The following communications were read:—

I. "Photo-chemical Researches.—Part II. Phenomena of Photo-chemical Induction." By Prof. BUNSEN of Heidelberg, and HENRY ENFIELD ROSCOE, B.A., Ph.D. Communicated by Prof. STOKES, Sec. R.S. Received November 27, 1856.

(Abstract.)

Chemical affinity, or the force which regulates the chemical combination of two bodies, is like all other forces, a certain definite quantity. Hence it is erroneous to say, that under different circumstances the same body can possess different affinities; more correctly we should say, that in the one case the bodies are able to follow the chemical attraction of their molecules, whilst in another case opposing forces render this combination impossible. These opposing forces may be considered as resistances similar to those exerted in the passage of electricity through conductors, in the distribution of magnetism in steel, and in the conduction of heat. We overcome these resistances when by agitation we increase the formation of a precipitate, or by insolation effect a decomposition.

We call the act by which these resistances to combination are lessened, and the formation of a chemical compound promoted, "chemical induction;" and we specify this as photo-chemical, thermo-chemical, electro-chemical, or idio-chemical, according as light, heat, electricity, or pure chemical action is the force which promotes the combination.

The phenomena of photo-chemical induction are particularly interesting, as affording starting-points from which we may gain a knowledge of this mode of action of affinity.

That on exposing a mixture of chlorine and hydrogen to the light the action does not commence to its full extent at once, was observed

by Draper in 1843. An explanation of this fact was given by the supposition that the chlorine underwent on insolation a permanent allotropic modification, in which state it possessed more than usually active properties. This explanation is, as we shall show, erroneous, and the whole phenomenon is caused by the peculiar action to which we have given the name of photo-chemical induction. When the standard mixture of chlorine and hydrogen is exposed to a constant source of light, no action is at first perceptible; after a short time, however, the action becomes visible, and gradually increases until a constant maximum is reached. Experiments made with different amounts of light from different luminous sources, showed that the times which elapsed from the beginning of the exposure until the maximum was obtained, varied very much, according to circumstances. In one case the maximum action was reached in fifteen minutes, in others after an exposure of three and four minutes. The first action was in one case visible after six minutes' insolation, in others after one minute, whilst in some experiments a considerable action was observed in the first minute.

The condition modifying the action of the induction which we first examined, was the action of the mass of the insolated gas. From various experiments, it was found that the duration of the induction increased with the volume of exposed gas (by constant amounts of light), and curves have been drawn, representing the increase of the induction for the various volumes of gas employed.

We next examined the dependence of the duration of induction upon the amount of light to which a constant volume of gas was exposed, and experiment showed—

1. That the time necessary to effect the first action of the photo-chemical induction decreases with increase of light, and in a greater ratio than the increase of light.

2. That the time which elapses until the maximum is attained also decreases with increase of light, but in a much less ratio.

3. That the increase of the induction proceeds at first in an expanding series, attains a maximum, and then converges when the true maximum action is attained. The law regulating the increase of the induction by increase of light, we have rendered visible by curves.

The results of these experiments suggested the question, Is this

condition of increased combining power, into which the mixture of chlorine and hydrogen passes by insolation, permanent, or is it confined to the time during which the gas is exposed to the light? In order to determine this question, the sensitive gas, which had stood for some time in the dark, was exposed to a constant source of light, and the time noted which elapsed before the maximum action was reached; the apparatus was then darkened for one minute, and then again insolated, and the time watched until the maximum action was again observed. These observations were repeated several times, each period of darkening being longer than the preceding. Thus conducted, the experiment led to the important conclusion, that the resistance to combination overcome by the influence of the light is soon restored when the gas is allowed to stand in the dark. Curves expressing the effect produced on induction by darkening, and by exposure to light, have been drawn.

We have explained the fact, that the mixture of chlorine and hydrogen does not combine in the dark, by the supposition of the existence of a resistance to combination which is overcome when the gaseous mixture is exposed to light. This resistance to combination can be increased by various circumstances. The presence of a very small quantity of foreign gas in the standard mixture of chlorine and hydrogen is sufficient to cause the resistance to be increased to a very great extent. An excess of $\frac{3}{1000}$ of hydrogen over that contained in the normal gas, reduced the action from 100 to 38.

In these experiments we have to do with the purest form of the so-called catalytic actions, to which the photo-chemical phenomena are closely related. The quantitative estimation of the relations which exist in the phenomena of contact, between the mass of the substance the time and other modifying conditions, has not as yet been possible, owing to the absence of any case in which these relations are exhibited in their simplest form. Our method of photo-chemical measurement points out a direction which promises to afford interesting results concerning these quantitative relations; but in this communication we restrict ourselves to the consideration of these phenomena in so far as they influence the action of photo-chemical induction, intending on a future occasion to enter more fully into the new field of research indicated.

The contact action of foreign gases is still more strongly seen in

the case of small quantities of oxygen. This gas, when present in quantities amounting only to $\frac{5}{1000}$ of the total volume of gas, diminishes the action from 100 to 4·7, whilst $\frac{13}{1000}$ reduced the action from 100 to 1·3. Excess of chlorine acted in a similar manner, though not to so great an extent, $\frac{10}{1000}$ of this gas reducing the action from 100 to 60·2, and $\frac{180}{1000}$ from 100 to 41·3. On examining the effect of small quantities of hydrochloric acid gas upon the induction maximum, we found, fortunately for the accuracy of the indications of our instrument, that an amount of $\frac{1 \cdot 3}{1000}$ of this gas does not produce any appreciable effect on the action of the induction. Uninsolated gas was found to act similarly on the normal mixture, the admission of $\frac{6}{1000}$ of non-insolated gas reducing the action from 100 to 55. Curves have been drawn, representing the relation between the action and the amount of foreign impurity introduced. Several series of experiments also showed that a mixture of chlorine and hydrogen, which was so nearly pure that no alteration of the maximum action was observable, was longer in attaining the maximum than the perfectly pure gas; hence the duration of the induction serves as an exact measure of the absence of all foreign gases in the standard mixture.

An explanation of the laws of photo-chemical induction derived from the above-mentioned experiments, might easily be found in the assumption that the chlorine or the hydrogen, or both gases, undergo upon exposure to light a change similar to that between common and ozonized oxygen, or that these two gases can, under certain circumstances, be invested with active, and, under other circumstances, with passive properties. If this hypothesis be true, each gas must undergo this peculiar modification when separately exposed to the action of the light. That this is not the case was shown by the following experiment:—The two gases were separately evolved, and each led through a long glass tube, in which they could be separately exposed to the action of diffuse and direct sunlight. After this exposure, the gases passed through a connecting tube into the apparatus, in which a constant source of light gave the duration of the induction. Thus alternately insolating and darkening the separated gases, we observed the effect on the gases subsequently mixed and exposed to lamplight. No difference was perceptible in the duration of the induction between the gases previously insolated and those

evolved in the dark. Hence we may conclude, that the light does not effect a permanent modification, either of the chlorine or hydrogen, but that the combination produced by the light must depend on photo-chemical action affecting only the increasing attractions of the chemically active molecules.

All the curves representing the increase of the induction under various conditions have a common form, and a point of flexure at which the maximum increase occurs. In order to determine whether this common property of the curves arises from the general mode of action of affinity, or whether the light plays an essential part, we have made experiments upon idio-chemical induction, *i. e.* action in which pure chemical attractions alone effect the alteration. For this purpose we employed a dilute aqueous solution of bromine with tartaric acid, which mixture, when left to itself in the dark, undergoes decomposition, hydrobromic acid being formed. By determining the amount of free bromine contained in the liquid at different times, we became acquainted with the rate at which the decomposition occurred. Analysis showed that the amount of hydrobromic acid formed was not the same in equal spaces of time; and curves representing this increase were found to have the form obtained for the photo-chemical induction. Hence the cause of this maximum increase appears not to lie in any peculiar property of light, but rather in the mode of action of affinity itself.

One of the many interesting applications of the laws of photo-chemical induction relates to the phenomena of photography. As an instance of this application we cite the remarkable observations of Becquerel, which induced him to assume the existence of certain rays which can continue, but not commence, chemical action. In order to explain the phenomenon observed by the French physicist, we do not need to suppose the existence of a new property of light, as the facts are easily explained by the laws of photo-chemical induction; and we are satisfied that these relations, which we have examined only in the case of chlorine and hydrogen, occur in a slightly modified form in other photo-chemical processes.

Having determined in this part of our investigation the most important phenomena of photo-chemical induction, we shall in the next section consider the laws which regulate the chemical action of light after the induction is completed.

II. "Observations on Glaciers." By JOHN TYNDALL, Esq., F.R.S., and THOMAS H. HUXLEY, Esq., F.R.S. Received January 15, 1857.

(Abstract.)

On the 6th of June, 1856, certain views were advanced by one of us on the origin of slaty cleavage, and soon afterwards his attention was drawn by the other to the observations of Prof. J. D. Forbes on the structure of glacier ice, as suggesting the idea that the ice structure might be due to the same cause as the slate cleavage. On consulting the observations referred to, the lecturer at once perceived the probability of the surmise, and the consequence was a joint visit for a few days to the glaciers of Grindelwald, the Rhone, and the Aar. The subject being a physical one, it was followed up by the physicist on his return from the Continent. Reading, reflection, and experiment extended the inquiry until it embraced the main divisions of the problem of glacial structure and motion; and the paper now submitted to the Society contains an account of the investigation.

The first division is devoted to the consideration of certain phenomena connected with the motion of glaciers. The power of glaciers to accommodate themselves to the sinuosities of the valleys which they occupy, and the resemblance of their motion through such valleys to the motion of a river, suggest ideas which find their clearest expression in the viscous theory of glacier motion, propounded by Prof. J. D. Forbes. Numerous appearances indeed seem to favour this notion. The aspect of many glaciers as a whole, their power of closing up crevasses, and of reconstructing themselves after having been precipitated down glacial gorges; the bendings and contortions of the ice, the quicker movement of the central portion of the glacier where it is uninfluenced by the retardation of the banks,—are all circumstances which have been urged with such ability as to leave the viscous theory without any formidable competitor at present. To these may be added, the support which the theory derived from its apparent competency to explain the laminar structure of the ice,—a structure which it is affirmed is impossible of explanation upon any other hypothesis.

Nevertheless this theory is so directly opposed to our ordinary experience of the nature of ice, as to leave a lingering doubt of its truth upon the mind. To remove this doubt it is urged, that the true nature of ice is to be inferred from experiments on large masses, and that such experiments place the viscosity of ice in the position of a fact rather than in that of a theory. It has never been imagined that the bendings and contortions, and other evidences of apparent viscosity exhibited by a glacier, could be made manifest on hand specimens of ice. In the present paper, however, this is shown to be possible. Spheres of ice are described as being flattened into cakes, and squeezed into transparent lenses. A straight prism of ice six inches long, is described as having been passed through a series of moulds augmenting in curvature, and finally coming out bent into a semi-ring. A piece of ice is placed in a hemispherical cavity and is pressed upon by a protuberance not large enough to fill the cavity, and is thus squeezed into a cup. In short, every observation made upon glaciers, and adduced by writers upon the subject in proof of the plasticity of ice, is shown to be capable of perfect imitation with hand specimens in the laboratory.

These experiments, then, demonstrate a capacity on the part of small masses of ice which has hitherto been denied to them by writers upon this subject. They prove to all appearance that the substance is even much more plastic than it has hitherto been supposed to be; but the real germ from which these results have sprung, is to be found in a lecture given by Mr. Faraday at the Royal Institution in 1850, and reported in the 'Athenæum' and 'Literary Gazette' for that year. Mr. Faraday then showed, that when two pieces of ice, at a temperature of 32° Fahr., are placed in contact, they freeze together by the conversion of the film of moisture between them into ice. The case of a snow-ball was referred to as a familiar illustration of the principle: when the snow is below 32°, and therefore dry, it will not cohere, whereas when it is in a thawing condition, it can be squeezed into a hard compact mass. During one of the hottest days of last July, when the temperature was upwards of 100° Fahr. in the sun, and more than 80° in the shade, a number of pieces of ice placed loosely together in a window in the Strand, were observed by one of us to be frozen together; and he subsequently caused pieces of ice to freeze together under hot water. Hence the thought arose,

that if a piece of ice, a straight prism for example, were placed in a bent mould and submitted to pressure, it would break, but the continuance of the force would bring its severed surfaces to reunite, and that thus the continuity of the mass might be re-established. Experiment, as we have seen, completely confirmed this surmise; the ice passed from a continuous straight bar to a continuous bent one; the transition being effected, not by a viscous yielding of the particles, but *through fracture and regelation.*

All the phenomena on which the idea of viscosity has been founded, are brought by experiments similar to the above into harmony with the demonstrable properties of ice. In virtue of these properties the glacier accommodates itself to its bed; crevasses are closed up, and the broken ice of a cascade, such as that of the Talèfre or the Rhone, is recompacted to a solid continuous mass. But if the glacier effects its movement in virtue of the incessant fracture and regelation of its parts, this process will in all probability be accompanied by an audible cracking of the mass, and thus a noise of decrepitation may be expected to be heard, which would be absent if the motion were that of a viscous body. It is well known that such noises are heard, and they thus receive a satisfactory explanation*.

* It is manifest that the continuity of the fractured ice cannot be completely and immediately re-established after rupture; it is not the same surfaces that are regelated, and hence the new contact cannot be perfect throughout. After rupture the surfaces of fracture will enclose for a time *capillary fissures*, and thus the above theory is in harmony with the known structure of glacier ice. Since the paper was presented to the Society, I (on January 30th) made the following experiments bearing upon this point:—A piece of ordinary ice was taken, and a cavity hollowed in it was filled with an infusion of cochineal; the ice was perfectly impervious to the liquid, which remained in it for half an hour without penetrating it in the slightest degree. A piece of the same ice was subjected to a gradually increasing pressure. Flashes of light were seen to issue from it at intervals, which indicated the rupture of optical continuity, and a low, and in some instances, almost musical crackling was heard at the same time. Relieved from the pressure, the ice appeared continuous to the naked eye, but a cavity being formed and the cochineal infusion placed within it, the coloured liquid immediately diffused itself through the capillary fissures, producing an appearance accurately resembling the drawings illustrative of the infiltration experiments of M. Agassiz on the glacier of the Aar.

To account for a "*bruit de crépitation*" heard upon the Aar glacier, M. Agassiz

The next division of the paper treats of the veined or laminar structure of glacier ice, which Prof. J. D. Forbes, in his earlier writings, compared to slaty cleavage. His theory of the structure is perhaps the only one which has made any profound impression, and it may be briefly stated as follows. Owing to the quicker flow of the centre of a glacier, a sliding of the particles of ice past each other takes place; in consequence of this sliding, fissures are produced, which, when filled with water and frozen in winter, produce the blue veins of the glacier. To account for the obliquity of the veins to the sides of the glacier, a drag towards the centre is supposed to take place, producing a differential motion which results in the formation of fissures. But at the centre of the glacier this drag towards it cannot be supposed to exist; and to account for the veined or laminated structure of the centre, which, under normal conditions, is transverse to the length of the glacier, it is supposed that the thrust behind meeting an enormous resistance in front, produces a differential motion in a direction approximating to the vertical, and that in consequence of this motion fissures are produced, which, when filled and frozen, produce, as in the other cases, the blue veins. In the present paper it is observed that the only *fact* connected with this theory, is that of differential motion in the direction of the length of the glacier. Beyond this, all is conjecture. It

refers to an observation which might be made on a fine day in summer, and which would show the air within the glacier ice escaping from its surface. M. Agassiz supposes the ice to be diathermanous, and that thus the sun-beams get through it and heat the air-bubbles which it encloses, causing the air to expand, rupture the ice, and escape in the manner observed. The observation is an interesting one, whatever difficulty we may find in the explanation. An experiment made to-day (January 31) appears to me to account for the observation in a satisfactory manner. Snow having fallen, I was early at work compressing it; and on removing a plate of it from the press, I noticed, as the surface melted, a sparkling motion produced by the escape of the air enclosed within the mass. To imitate the action of the sun upon the glacier, an iron spatula was heated, and on bringing it near the surface of the compressed snow, the jumping of the surface, caused by the issue of air through the film of water which covered it, was greatly augmented. On removing the spatula the motion subsided. To a similar action on the part of the sun melting the surface of the glacier, and thus liberating by degrees the air-bubbles with which the ice is filled, the observation of M. Agassiz is in all probability to be referred.—J. T.

is by no means established that the cold of winter can reach to depths sufficient to produce the blue veins, which are affirmed to be a portion "of the inmost structure" of the glacier. Again, the lamination in some cases presents itself in the form of transparent lenticular masses, imbedded in the general substance of the white ice. The differential motion referred to would be mechanically inadequate to produce detached cavities corresponding to these masses, which vary greatly in size, and in some cases accurately resemble the greenish spots seen in slate rock when a section perpendicular to the cleavage is exposed. Further, as the fissures are produced by the motion of the glacier, and as this motion takes place both in summer and winter, it is to be inferred that they are formed at both seasons of the year. But if formed in winter, they cannot be filled and frozen that season for want of water; and, if formed in summer, they cannot be frozen while summer continues for want of cold. Hence, at the end of each summer there ought, if the above theory be correct, to be a whole year's unfrozen fissures in the ice. Such fissures could not possibly escape observation if they existed; but they never have been observed,—hence it is inferred that they have no existence. With regard to the drag towards the centre of the glacier, it is observed, that if such a drag existed, the centre would ultimately absorb the sides, unless the loss were made good by a motion in some part of the glacier, from the centre towards the sides, which would be in opposition to the theory. Experiments are described which prove that no such drag exists, and the actual motion observed is reduced to its elementary mechanical principles. With regard to the transverse lamination at the centre of the glacier, the hypothesis involves, among others, the difficulty of supposing that fissures can be formed in a mass assumed to be viscous, at right angles to the direction of an enormous pressure to which the mass is subjected.

An attempt is next made to apply the theory of slaty cleavage, propounded by one of the authors, to the laminated structure of glacier ice. It is shown that this lamination, like that of slate rock, is always approximately at right angles to the direction of maximum pressure;—that local circumstances which give rise to a violent thrust, produce at the same time a highly developed lamination. When two confluent glaciers unite to form a single trunk, the effect

of their mutual thrust is to develope the veined structure along their line of junction. This is illustrated in the case of the Aar and other glaciers; and experiments are described which show the mechanical condition of such glaciers, and that the veined structure of the ice, which sometimes rises to a case of "true cleavage," occurs at the precise places where the compression theory would lead one to expect it. The lenticular structure before referred to, the obliquity of the veins to the sides of the glacier, the transverse lamination at the centre, the relation of the blue bands to the crevasses, are all in perfect harmony with this theory. Indeed, unless we suppose the ice of glaciers to be perfectly homogeneous, mechanically speaking, we must infer that under pressure some portions will be rendered more compact than others, and the veined structure is the natural consequence*.

Finally, the well-known phenomenon of "dirt-bands" upon the surfaces of glaciers is considered, and an explanation of those bands, as seen upon the glaciers of Grindelwald and the Rhone, is attempted. On the former glacier the bands were particularly well developed;

* Since the presentation of the paper, I have tried to reproduce the veined structure on a small scale by compressing snow. In some cases the section of the mass perpendicular to the surfaces on which the pressure was exerted, exhibited in a feeble but distinct manner an appearance the same in kind as that of the veined structure of the glacier ice; stripes more transparent than the surrounding ice were observed at right angles to the direction of pressure. I have also succeeded in impressing upon a perfectly transparent prism of ice a cleavage which certainly surpassed my expectations. The cleavage, as in the case of the glacier and of slate rock, is perpendicular to the direction of pressure. On placing a specimen of the squeezed mass before a highly competent judge, he at first imagined it to be a piece of gypsum. The full details of these experiments shall be communicated in due time to the Royal Society. The case, then, as regards slaty cleavage and the structure of glacier ice, stands thus:—The testimony of independent observers proves that they are both laminated at right angles to the direction of pressure; and the question occurs, Is the pressure sufficient to produce the cleavage? Experiment replies in the affirmative. I have reduced slate rock to an almost impalpable powder, and reproduced from it the lamination by pressure. In the glacier we find equally the cleaved structure associated with pressure, and the above experiments prove the sufficiency of the pressure to produce the structure. By combining the conditions of nature, we have produced her results; and it may perhaps be questioned whether a theory is capable of more convincing proof than that thus furnished.—J. T.

and a portion of the glacier where they did not exist was presented to us simultaneously with the bands upon another portion. Their proximate origin and final completion were thus before us. The explanation offered in the paper is, that the dirt scattered by avalanches and winds over the upper regions of a glacier is redistributed by the passage of the glacier down a cascade where the ice is shattered, and the dirt broken up into detached patches. On reaching the bottom, where the force becomes one of compression, the patches of dirt are squeezed longitudinally and drawn out laterally, being thus converted into stripes of discoloration, which, owing to the speedier motion of the centre of the glacier, are convex towards its lower extremity. On consulting the map of Prof. Forbes, it will be seen that the dirt-bands commence at the base of the ice-fall of the Talèfre, while none exist above the fall. Those shown on the Glacier du Géant, we are led to infer, commence at the base of the Cascade of la Noire, which, however, is not sketched on the map. The theory of Prof. Forbes is, that a glacier, throughout its entire length, is composed of alternate segments of hard and porous ice; that the dirt is washed from the former, but finds "a lodgement" in the latter, thus giving rise to the phenomenon of the bands. We are unacquainted with the experiments on which this theory is founded, and have only to state, that the bands which we have observed seem accounted for by the simple explanation given above. In the paper, experiments are described illustrative of this view, and in which the bands are accurately exhibited on a small scale.

In the course of the inquiry much assistance was derived from the use of troughs of various shapes and sizes, through which mud, formed by the mixture of finely-sifted pipeclay and water, was permitted to flow. Coloured circles being stamped upon the mud, from the distortion of these the character of the forces, whether compression or tension, acting upon the mass was inferred. It is needless to say, that this mode of experiment does not pledge the authors to the view that ice is similarly constituted to mud. Where the mud *stretches*, the ice *breaks*; and the experiments were mainly instituted for the purpose of examining the relation of the forces acting upon a glacier to its directions of fracture.

It may, however, be urged, that, after all, the foregoing experiments do not prove the viscous theory to be wrong. The mere fact of

bending a prism of ice by fracture and regelation, does not prove that ice is non-viscous. This is perfectly true; nor is it conceived that the onus rests on us to prove the negative here. All that is claimed for the foregoing experiments is the referring of certain observed phenomena to a true cause, instead of to an imaginary one. An illustration may perhaps serve to place this question in its true light. By Newton's calculation, the velocity of sound through air was one-sixth less than what observation made it; and to account for this discrepancy he supposed that the sound passed instantaneously *through* the particles of air, time being required only to accomplish the passage from particle to particle. He supposed the diameter of each air-particle to be $\frac{173}{916}$ths of the distance between two particles, *and nobody ever proved him wrong*. Still, when Laplace assigned a *vera causa* for the discrepancy, the hypothesis of Newton, and other ingenious suppositions, were discarded. The proof indeed in such cases consists in the substitution of a *fact* for a *conjecture*; and whether this has been done in the case now before us, the intelligent reader must himself decide.

January 22, 1857.

Dr. W. A. MILLER, V.P., in the Chair.

The following communications were read:—

I. "On some of the Products of the Destructive Distillation of Boghead Coal."—Part I. By C. GREVILLE WILLIAMS, Esq., Assistant to Dr. ANDERSON, Professor of Chemistry in the University of Glasgow. Communicated by Dr. SHARPEY, Sec. R.S. Received November 25, 1856.

(Abstract.)

The paper, of which the following is a brief abstract, constitutes the first part of the author's examination of the hydrocarbons con-

tained in boghead naphtha. In it he gives the results of his experiments on that portion of the fluid which resists the action of monohydrated nitric and sulphuric acids. He had previously stated the fact of his having obtained a substance possessing the composition and vapour-density of butyl*, and had expressed a belief that he should succeed in isolating not only that radical, but also propyl, amyl, and caproyl. The composition of the radicals varies so little, that to determine the boiling-points it was necessary to take the density of the vapour of all those fractions which distilled anywhere near their known boiling-points; and in each case he regarded that fraction which gave the nearest result as representing the boiling-point of the radical as obtained from the source mentioned.

Propyl presented itself under the form of a colourless, very mobile fluid, having a pleasant odour, and boiling at 68° C. At 18° it had the very low density of 0·6745. Combustion gave results agreeing closely with the formula

$$C^{12} H^{14} = 4 \text{ vols.},$$

confirmed by a determination of the density of its vapour by Gay-Lussac's method, which gave 2·96, theory requiring 2·97. Propyl had not been previously obtained.

Butyl from the Torbane-hill mineral distils at 119°, and has a density of 0·6945 at 18°; its analysis coincided with the formula

$$C^{16} H^{18}.$$

The vapour-density was found to be 3·88, theory requiring 3·94.

Amyl distilled at 159°, and had a density of 0·7365 at 18°. On analysis, numbers were obtained agreeing perfectly with the formula

$$C^{20} H^{22}.$$

The vapour-density was found to be 4·93; theory requires 4·91.

Caproyl boiled at 202°; its density at 18° was 0·7568. The results of analysis accorded with the expression

$$C^{24} H^{26},$$

which indicates a vapour-density of 5·87, while experiment yielded 5·83.

The experiments detailed in the paper appear to demonstrate the

* Proceedings of the Royal Society, vol. viii. p. 119.

radical nature of the hydrocarbons, and to negative the assumption of their being homologues of marsh-gas.

The paper concludes with a description of a method, by which, where numerous vapour-density determinations are to be made, the necessity is avoided of refilling the balloon with water or mercury in order to determine the residual air.

II. "On the Optical Characters of certain Alkaloids associated with Quinine, and of the Sulphates of their Iodo-compounds." By William Bird Herapath, M.D., in a Letter to Professor Stokes, Sec. R.S. Communicated by Professor Stokes. Received January 8, 1857.

You will probably recollect that I sent you some time since a small portion of an alkaloid, which at that time was called quinidin in Germany, but it has since been distinguished from it and named cinchonidin. You then examined it for epipòlism or fluorescence, and you pronounced the opinion that it possessed this property only in a minor degree, and you imagined that this arose from the presence of a small per-centage of α-quinine.

I have since obtained, through the kindness of Mr. J. E. Howard, specimens of the perfectly pure alkaloids quinidin and cinchonidin, and find that quinidin, which I can now identify as the β-quinine of Von Heijningen, possesses the phenomenon of fluorescence or epipolic dispersion as powerfully as α-quinine; whilst cinchonidin, if perfectly pure, is devoid of it altogether; and recent experiments have shown me that a small per-centage of quinidin was the cause of the epipolic dispersion found by you in the specimen of cinchonidin sent by me.

It may be as well to state that the cinchonidin tested by water of chlorine and ammonia gave no evidence of green tint, which it would have done if only $\frac{1}{5000}$dth part of either α-quinine or quinidin had been present, according to some recent experiments of my own.

I have also found that 1 gr. of pure quinine or quinidin in 35,000 of water will give an evident "*epipolic*" appearance; whilst when diluted with 70,000 grs. of water we have still very evident appear-

ances of "fluorescence" upon the perpendicular wall of the glass vessel exposed to the incident light; whilst a bluish milkiness of "internal dispersion" may be seen when 1 gr. of either alkaloid is diluted with 700,000 grs. or 10 gallons of distilled water, well acidulated in all these cases with sulphuric acid.

Some other interesting results have followed from these investigations. When quinidin is dissolved in an excess of diluted sulphuric acid, and the solution mixed with about twice its bulk of spirit, and warmed to 130° F., and tincture of iodine then added in sufficient quantity, and subsequently set in repose, beautiful red acicular crystals are deposited; these, upon recrystallization from rectified spirit, acquire an increased size, become beautiful quadrilateral prisms, leaving a deep garnet-red by transmitted light, and possessing a clear bluish-purple reflected tint; they are optically doubly absorbent in a slight degree, and transmit a brownish-orange body-colour when polarized perpendicular to axis. The primary form appears to be a rhombic prism, and as far as my present analyses go, appears to possess centesimally the following composition:—

Iodine	39·665
Sulphuric acid	6·273
Carbon	32·890
Hydrogen	3·960
Nitrogen	4·400
Oxygen	5·040
Water	8·504
	100·712

The excess arises from the hydrogen twice calculated in water of crystallization.

These numbers agree very closely with the formula

$$(C^{35} H^{19} N^{2} O^{4} + I^{2}) SO^{3} HO + 5HO,$$

and prove it to be the sulphate of iodo-quinidin, very analogous to the optical quinine compound, yet differing materially in its optical properties.

There is another alkaloid frequently associated with quinidin, which also crystallizes from spirit in the prismatic form like quinidin and cinchonidin, but is another example of epipolism or fluorescence.

Its iodo-sulphate is deep olive-green in its reflected tint, orange-yellow by transmitted light, and possessing in an eminent degree optically doubly absorbent powers, thin laminæ being quite black, but still thinner ones give a bistre-brown "body-colour" when polarized perpendicular to axis.

This alkaloid was also furnished me by the kindness of Mr. Howard, but has not yet been sufficiently purified, or in the quantity necessary to give certain results.

It possesses one very peculiar property. When dissolved in chloroform and evaporated spontaneously on glass, the gummy and uncrystalline residue, mounted in Canada balsam, at once shows a deep blue-green, epipolic, or fluorescent appearance.

Pure cinchonidin does not possess epipolic dispersion and does not become green by chlorine-water and ammonia; and when it is dissolved in acetic acid or chloroform and the fluid is exposed on glass plates to spontaneous evaporation, beautiful crystals in circular spots or drusæ develope themselves, which under polarized light exhibit black crosses and white or coloured sectors.

These appearances are not exhibited by pure quinine or true quinidin (β-quinine), both of which give a gummy, uncrystalline, and perfectly transparent residue.

Pure cinchonidin, thus optically and chemically distinguishable from either quinine or quinidin, is still further remarkable for producing with sulphuric acid and iodine an optical doubly absorbent compound of intense power, even equal to the sulphate of iodo-quinine compound; these crystals are very similar in form to my artificial tourmalines, and have long been mistaken by me for them; even at present I can only distinguish them by the tints in reflected light and the complementary body-colour, viz. whilst sulphate of iodo-quinine gives a cantharidin-green reflected tint, and a pink, ruby-red, reddish-brown or black body-colour when polarized perpendicular to the axis, according to the thickness of the plate examined, the sulphate of iodo-cinchonidin is golden-green by reflected light, and gives a sky or indigo-blue or black "body-colour" when polarized perpendicular to the axis. I have not yet made sufficient chemical analyses of this substance to enable me to decide on its formula, but I have obtained 39·307 per cent. iodine and 8·864 per cent. sulphuric acid, which sufficiently indicate a chemical difference in constitution

from the sulphate of iodo-quinine, which, it may be remembered, contains 32·609 per cent. iodine and 10·61 per cent. sulphuric acid.

I hope soon to present these results in more detail when sufficient leisure is afforded me for the purpose.—W. B. H.

January 29, 1857.

Major-General SABINE, Treas. and V.P., in the Chair.

The following communications were read:—

I. "On the Nervous System of *Lumbricus terrestris*." By J. LOCKHART CLARKE, Esq., F.R.S. Received Dec. 18, 1856.

(Abstract.)

In the summer of 1855, with the view of throwing some light on other researches in human anatomy, in which he was already engaged, the author undertook some anatomical inquiries on the nervous system of Invertebrata; but finding them occupy more time than he could spare, he was compelled to relinquish the pursuit after having made many interesting but desultory observations on various animals. As he had proceeded, however, to a considerable extent with the nervous system of *Lumbricus terrestris*, and discovered in it much that is important and was hitherto unknown, he has thought it expedient to resume and complete this portion of the subject without further delay.

Before treating of the nervous system it was necessary,—in order to show the proper functions of many of its parts,—to give some account of the organs of prehension, deglutition and digestion; and as these are insufficiently explained elsewhere, the author has described them entirely from his own dissections and observation.

The first anterior segment is a conical or nipple-shaped projection inserted behind into the upper fifth of the second segment, or first ring. Its dorsal surface is covered, except in the centre, by concentric laminæ and irregular masses of pigment-granules, which are interspersed with large, peculiar and nearly pellucid cells. Its under part forms

a soft and delicate pad, or upper lip, and is continuous at the sides with the inferior half of the second segment, or under lip, to complete the oral orifice from which the mucous membrane of the mouth is reflected inwards. The mouth is a wide tube surrounded by a delicate muscular coat, and attached to the outer tube, or rings, by fine muscular bands. Behind, it dilates into a capacious heart-shaped sac, of which the roof or upper wall is covered by a thick oval muscular mass. The outer portion of this mass is divided into distinct, radiating, digital muscles which connect it on all sides and are continuous with the longitudinal muscles of the rings. Its inner surface projects anteriorly into the cavity of the pharynx, in the form of a thick circular disc or sucker, surrounded by loose folds of mucous membrane. Opening into the sides of the mouth and pharynx are two or three sets of salivary glands, which consist of convoluted tubules, resembling those of Lepidopterous insects: these glands have not been hitherto detected in *Lumbricus terrestris*. The pharynx contracts into a comparatively narrow œsophagus, which in its turn dilates into a capacious crop; and this immediately opens into a cylindrical gizzard composed of a ring of cartilage, with an external muscular coat, and a lining of mucous membrane. A long straight and narrow intestine extends through the rest of the body, and is covered throughout with yellow, follicular, hepatic glands in circles corresponding to the segments.

Nervous System.—The central organs of the nervous system consist chiefly of a bilobed cephalic ganglion, and a double chain of subventral ganglia extending through the whole length of the body. The lateral lobes of the cephalic ganglion are pyriform, and united by their broader ends in the mesial line. The small end of each divides into two nerve-trunks, of which one forms the root of its cephalic nerves, and the other, the pharyngeal crus, which curves round the side of the pharynx to join the first subventral ganglion. Each crus gives off eight or nine branches. The first four or five arise from the under part of its anterior half, and immediately enter the upper surface of a minute and delicate cord-like chain of ganglia, the enlargements of which correspond to them in number and size. This highly interesting structure lies on the side of the pharynx, concealed beneath the crus. The breadth of its first ganglionic enlargement in a good-sized worm, was the $\frac{1}{200}$th of an inch; that of the

last the $\frac{1}{130}$th; the pharyngeal crus, where their roots come off, was $\frac{1}{100}$th of an inch in diameter. Each border of the chain gives off several trunks of considerable size, which immediately communicate to form a continuous plexus. The part of the plexus on the inner side is much the larger, and supplies anteriorly, the muscular and mucous coats of the mouth as far as the lips; and posteriorly, the pharynx and suctorial disc; uniting in both directions with its fellow of the opposite side. The outer part supplies the muscular bands and salivary tubules. From the pharynx, the plexus descends along the side of the œsophagus, lying on the abdominal vessels, and communicates with minute filaments from the nerves of the subventral ganglia.

The whole of this little chain with a large portion of its plexus and the wall of the pharynx on which it lies, was removed and examined under a $\frac{1}{8}$th-inch object-glass, when a beautiful and unexpected appearance was observed. The under surface of the *entire* chain—cords as well as ganglia—was covered with a lamina of round, oval, and pyriform cells; and on its upper surface a row of cells of the same kind was found along each border. At every point of communication between the branches which form the plexus, a minute ganglionic enlargement was observed, from which new branches proceeded to form other enlargements of the same kind. Every branch communicated by loops with those adjacent, and by transverse fibres with those of the opposite side, giving to the ganglionic points a kind of stellate appearance. In these microscopic ganglia, the nerve-cells, similar to those of the chain, were accumulated chiefly about the angles, along the borders, and extended some distance into the principal trunks; but very few could be seen in connexion with nerve-fibres, which ran around and between them, however, in an intricate manner. As the plexus extended from the chain, the ganglionic points diminished in size, while the smaller branches given off from the trunks increased in number, and communicated like a capillary network. At the same time the ultimate fibres became paler, flatter, more parallel, and acquired nuclei like those of cells. This was particularly observed in those distributed to the mucous membrane. The above observations were repeated on nearly forty different specimens.

On considering the parts which it supplies, this little chain appears to combine the office of a sympathetic with certain other functions

which in many Invertebrata are entrusted to separate and special centres;—such as the labial, pharyngeal, and visceral ganglia in Cephalopodous and Gasteropodous Mollusca, and the separate parts of the stomato-gastric system of insects, which, although derived from different sources, are in intimate communication with each other. The lateral ganglia in insects have the same position as the little chain of *Lumbricus*, on the side of the pharynx, which, according to Mr. Newport, is supplied entirely by them; they arise, however, wholly from the cephalic ganglion, while the chain in *Lumbricus* has just been seen to take its origin both from this and the pharyngeal collar; but then, in orthopterous insects, the gangliated recurrent nerve, which is always in intimate connexion with the lateral ganglia, arises entirely from the pharyngeal crus; and the fact has been observed by Burmeister, Brandt and Müller, that in some other orders these two parts, in regard to size, are in the inverse ratio of each other. In Crustacea also, the whole of the pharyngeal, gastric and visceral nerves take their origin from the crura, as was first shown by Audouin and Milne-Edwards.

The second set of nerves from the pharyngeal collar come off from its posterior half, and communicate with each other by loops before they leave it. The first and largest sends some filaments to the muscular bands of the mouth, upon which they communicate by evident but slight dilatations with the plexus of the pharyngeal chain; and after supplying the muscles of the anterior segments, are lost in the integument of the lower lip. The rest take nearly the same course. But what is extremely interesting, the roots of this set—at least of the first and second branches—are continuous across the crus with those of the former set which belong to the pharyngeal chain; and many of their fibres may be traced not only into its ganglia, but through the trunks which proceed from their opposite sides to form the pharyngeal ganglionic plexus; so that the nerves distributed to the labial muscles and integument of the outer tube, and those which supply the inflected oral and pharyngeal tube, are in direct continuity, not only at their peripheral extremity, but at their roots also, through the common centre which presides over the whole of the digestive apparatus. A similar connexion will be seen to exist with regard to the cephalic nerves.

The subventral chain is a double cord gangliated at short intervals

by the addition of vesicular substance. Anteriorly the cords are separate and continuous with the pharyngeal crura of their respective sides; but through the rest of their course they lie in close contact along the middle line. The ganglionic enlargements vary somewhat in size, shape, and approximation at different parts. The vesicular substance is on their under surface, and consists of about two strata of cells continuous in a lamina across both cords. Along their borders, however, the cells form a thicker layer or column, which extends for some distance along the intervening cords. In form and general appearance the cells are similar to those of the pharyngeal chain, but many of them are larger. Those of the first ganglion extend into the lower parts of the crura, and are continuous behind with the lamina of the second. Each ganglion gives off from its sides two pairs of nerves, which, after sending some filaments to the septa and muscular bands, supply the longitudinal, oblique and circular muscles of the rings. Midway between the ganglia, the intervening cords give off a single pair, which are distributed to the deep muscles on each side. Within the ganglia the roots diverge in three different ways:—1, longitudinally; 2, transversely; and 3, to the grey or vesicular substance. The first or longitudinal form a large portion of the nerves, and run in equal numbers in both directions—backwards and forwards,—along the whole breadth of the corresponding cord. In their course, some of them, near the border, separate in succession from the rest and enter the lateral columns of cells; others proceed as far as the next nerve, with the roots of which they form loops, and pass out, while the rest continue onwards and, perhaps, in succession form similar loops with distant nerves. In former communications to the Royal Society, the author has shown that the same kind of arrangement exists in the spinal cord of Man and Mammalia.

The second or transverse order of fibres are less numerous, and in general less distinct than the last. They proceed from the middle of each opposite root, and cross the cords directly; but some of them, on reaching the opposite cord, turn round in both directions, and run with its longitudinal fibres. In front of the first ganglion, in which they are unusually distinct, a separate band unites the roots which descend from the branches arising from the opposite crura of the pharyngeal collar.

The third order of fibres, or those distributed to the vesicular substance, spread out in all directions, but always—except in the lateral layers or columns—beneath the superficial stratum. After nearly fifty separate examinations, with all the resources of the microscope, the author has not been able, in more than two or three instances, to trace an undoubted continuity between the cells and nerve-fibres. Fibres in abundance may be seen in connexion with the cells, but the greater number of these are not nerve-fibres. Nevertheless, there is reason to believe that such a connexion does frequently exist, but is obscured by certain peculiarities of structure. Still it is quite certain that a vast number of fibres pass by or around the cells near their origin, and many often appear to terminate in loops.

Cephalic Ganglion.—This rests on the commencement of the pharynx, beneath the dorsal part of the third ring. Each lobe is a pyriform sac, which is very thick and convex posteriorly, where it is partially separated from its fellow by a deep notch. This convex portion is opaque-white, and filled with a mass of semifluid granular substance, and oval, round and pyriform cells, of various sizes, but often very large. Some of the latter kind are exceedingly elongated. The anterior half, by which the lobes are joined, is merely *lined* with a *lamina* of cells, and only at its upper part, its under side having a cell here and there. The *interior* of this portion is entirely *fibrous*, and consists of a broad transverse commissural band derived from the pharyngeal collar, and of fibres from the roots of the cephalic nerves. Each crus of the collar enters its lobe on the *under* side. Some of its fibres curve backwards to the convex vesicular mass; others ascend to—perhaps partly terminate *in*—the cells near the roots of the cephalic nerves; and the rest cross transversely as the broad band, to be continuous in front of the notch with that of the opposite crus. The cephalic nerves are attached to the *upper* part of the ganglion. Many of their roots cross transversely with the crural band, to form loops with those of the opposite lobe. Decussating these, a considerable number run down the pharyngeal crus, and enter the pharyngeal chain of ganglia through its first and second roots, at least,—perhaps through all,—and probably form loops with the other set of branches of the crus. The remaining fibres of the cephalic nerves spread through the vesicular substance, partly describing curves and undulations in the corresponding lobe, and com-

municating in part with the other in the mesial line, where they form a kind of indistinct decussation in front of the notch.

Distribution of the Cephalic Nerves.—Their roots on each side immediately separate into two trunks, a lower and upper. The former runs above the mouth, to the under side of the first conical segment, or upper lip. Here it divides into several branches, which supply its muscular bands, and then terminate in the integument as a plexus, which appears to communicate with that from the first enlargement of the pharyngeal chain, spread over the tubular mouth, which is itself continuous with the upper lip. The upper trunk proceeds directly to the corresponding part of the same segment, and there divides into two branches, of which one in particular, after running the course of the pigmentary laminæ, and giving off a series of short filaments, terminates at the point, beneath the integument.

In the pigmentary laminæ the nerves form an intricate plexus, and the impression was that many of their ultimate fibres end in loops. They were never seen to be directly connected with the large clear cells scattered through the substance. Nor is there any ground for conjecture with regard to the office of these cells: perhaps they are intended for the transmission of light. From the structure of the segment and the distribution of its nerves, it is not unreasonable to think that its upper surface may be instrumental in the perception of diffused light; and that its under surface and point may be subservient not only to the sense of touch, but perhaps also to that of smell, in a low degree. That it forms an important organ of search, is pretty evident from the manner in which it is projected alternately forwards and from side to side, as the animal advances in its course.

The upper side of the cephalic ganglion corresponds to the under side of the subventral, and several points of resemblance are pointed out between the two kinds of centres. Each pharyngeal crus is shown to be a compound structure, composed of different sets of connecting fibres,—1, between its own nerves, which supply, on the one side, the cephalic portion of the outer tube, and on the other, the corresponding part of the alimentary tube reflected inwards from the former; 2, between these nerves and their fellows of the opposite crus, across the front of the first ganglion; 3, between the same nerves and the cephalic ganglion; and 4, between the cephalic

ganglion and the same side of the whole subventral chain. Now there is this point of *difference* between the two kinds of centres compared together,—that while the last-mentioned set of fibres on the one side is continuous with that on the other, as a transverse band through the cephalic ganglion, the subventral cords, although continuous with these on their respective sides, form no such connexion with each other across the lateral halves of their own ganglia, but run parallel and directly *backwards* through them. Such a communication, however, is established for the latter, individually, by the transverse fibres of their *own nerves*; and just as these fibres unite the lateral halves of each *separate* ganglion, *independently of the cephalic*, so do the last-mentioned set of fibres of the crura connect together the two lateral halves of the *entire chain in* and *through* the cephalic ganglion, which is their dominant and controlling centre.

Two parts of the human brain may be compared to this transverse cephalic band. One is the arched and commissural band of fibres prolonged through the corpora quadrigemina, from the upper and inner part of the fillet on each side. But the outer part of the fillet turns forwards and upwards beneath the corpus geniculatum internum and optic tract, to enter the optic thalamus. It is not improbable, therefore, that some of the fibres of the tract may descend along this portion of the fillet, to form loops with the roots of the fifth nerve, over which it passes, since in *Lumbricus* it has been seen that many of the roots of the cephalic nerves run down the pharyngeal crus to form loops with others to which it gives origin. In a former memoir by the author, it was shown that some of the roots of the spinal accessory nerve reach the anterior grey cornu and mingle, perhaps pass out with, the spinal roots; and he has since observed the equally interesting fact, that the same nerve forms a similar connexion with the vesicular nucleus of the hypoglossal, which may be considered a representative of the anterior spinal. The spinal-accessory, therefore, takes it origin from at least three different sources,—from its own nucleus, and from the nuclei of the hypoglossal and anterior spinal nerves. The peripheral communications of both the former with the latter nerves in the cervical plexus is well known. The author believes he has also made out an intimate connexion by loops between at least the portio intermedia of the

seventh and the large root of the fifth nerves within the substance of the human medulla.

The other part of the human brain which is analogous, or homologous with the cephalic band of *Lumbricus*, is the corpus callosum. Gall and others have thought that the fibres of this structure arise from the grey substance of the hemispheres; while some have endeavoured to show their continuity with those of the crura cerebri. Now it is quite certain that in the cephalic ganglion of *Lumbricus*, a large proportion of the commissural fibres are directly continuous with those of the pharyngeal crura; and there are appearances which favour the conclusion that some of the latter are confined to the lobe on their own side. From *analogy*, then, we may infer, that while a large portion of the crura cerebri are directly continuous with the corpus callosum, some of their fibres *probably* terminate in the cerebral convolutions of the corresponding side.

From what has been shown, it is evident that the communications between the roots of nerves are more intimate and extensive than they were hitherto believed to be; for it has been seen that the roots not only of every spinal nerve, but of every other in the system, communicate with those which *correspond* on the *opposite*, and with those which are *adjacent* on the *same* side. Of the cephalic with the two sets from the pharyngeal collar, and of the latter with each other, the connexions are particularly interesting, and may serve as guides to future investigations on other forms of the nervous system.

By experiments that were made on the living worm, it is shown that the pharyngeal chain of ganglia are independent of the other nervous centres, although subject to their influence, and are not only competent of themselves to preside over the complicated movements of the suctorial pharynx and mouth, but appear also to be centres of reflex action.

The present memoir concludes with some observations and remarks on the ganglionic cords of other Invertebrata.

II. "An Account of the two Methods of Reproduction in *Daphnia*, and of the Structure of the 'Ephippium.'" By John Lubbock, Esq., F.G.S. Communicated by Charles Darwin, Esq., F.R.S. Received December 22, 1856.

(Abstract.)

In this paper the author describes the male organs and the structure of the Ephippium in the genus *Daphnia*, and the double method of reproduction by agamic and ephippial eggs. The author calls the non-ephippial eggs agamic, but it is possible, though not probable, that the ephippial eggs may be agamic also. In the male *Daphnia* there are two small papillæ above the posterior claws, but on the ventral side of the anus, and on these being compressed, two streams of minute rod-like bodies, with movements so gentle as to be scarcely visible, will be seen to issue, one from each papilla. Nothing similar has ever been observed in the female; nor has any other sort of spermatozoa ever been met with. These male organs have never been described before.

The author then proceeds to describe and figure the two sorts of eggs in their earlier stages, which have not yet been mentioned by any naturalist. The ephippial eggs differ from the agamic in their determinate position and number. As a general rule, that is to say, in seventeen cases out of twenty-three, the author has remarked that ephippial eggs commence and are developed to a certain point.

The development is as follows. One of the ovarian cells, always at the posterior part of the ovary, swells a little, and becomes a germinal vesicle; round it are deposited a number of brownish granules, while the other cells which may at first have existed in the same ovarian mass cease to be visible. The deposition of dark granules, in thirty-seven cases out of forty, after proceeding to a certain point, ceased, and the embryo egg gradually disappeared. In the other three cases it increased, and at length formed a dark mass on each side of the intestinal canal. The author in two cases observed the ephippial eggs pass from the ovary into the receptacle.

The ephippium has been described by Strauss with considerable accuracy, but he has been more or less misunderstood by all subsequent writers on the subject, and no one has explained the homologies or connexions of the inner valve. The ephippium itself is

a locally altered portion of the carapace; the outer valve of the ephippium being a part of the outer layer of the epidermis, and the inner valve the corresponding part of the inner layer. In consequence of this arrangement, the inner valve of the ephippium, containing the ephippial eggs, is not attached by the hinge to the outer valve, as has been generally stated, but actually lies at first in the receptacle formed by the new carapace. The ephippium is cast with the rest of the skin, from which however it soon becomes detached, and continues to form an efficient protection to the eggs until they are hatched. These eggs probably require to be fertilized, but this fact is not completely proved. With one exception, whenever the author observed ephippia, he could also find males; and, generally speaking, the numbers of each were in proportion to one another. Impregnation is not, however, absolutely necessary to the production of ephippia, as the author has now in his possession three ephippia, formed by isolated females. It remains to be seen whether young will be developed from these or not.

The early stages of the agamic egg are very similar to those of the ephippial egg, and consist of the enlargement, in the front part of the ovary, of one of the ovarian cells, which then becomes a germinal vesicle, and the deposition round it of granules, with the addition in this case of oil-globules. This process continues, the other two or three cells which may have existed in the same ovarian mass gradually disappear, and there is thus formed an egg-like mass, consisting of a germinal vesicle, minute dark granules, and large oil-globules. When the growth is nearly completed, the vitelline membrane is added. This is at first very delicate, but after deposition in the receptacle soon becomes hard. The ovarian eggs of *Daphnia*, as well as those of *Cypris*, never contain round masses like those of *Aphis* and *Musca*; but after their entry into the receptacle, yolk masses are found, homologous with those present at the corresponding periods in *Phryganea**. The eggs when laid are about $\frac{20}{2000}$ of an inch in diameter; they gradually become $\frac{26}{2000}$, when the vitelline membrane splits and falls off, and the young animal is hatched. Far, however, from resembling its parent at this time, the young Daphnia

* The round balls described by Herold in the ovarian eggs of *Bombyx*, appear to be of a different nature, and homologous with the Nahrungsdotter mentioned by Carus in spiders' eggs and the oil-globules of *Daphnia*.

is a spherical bag, inside which the formation and development of the new organs is rapidly progressing*. Instead therefore of undergoing no metamorphosis, the young Daphnia only assumes the well-known characters of the genus after the first changes of skin. The author proceeds to compare this phenomenon with a similar one observed by Mr. Spence Bate in *Gammarus*, by Prof. Huxley in *Mysis*, by Dr. Cohn in *Sphæroplea*, in many Annelids, and in the interesting entozoon *Monostomum mutabile*. The young Daphnia attains a length of ·025 inch before it leaves the receptacle of the mother, but the length of time during which it remains therein varies according to the temperature. The author has never met with an exception to the rule noticed by preceding writers, that unisexuality is characteristic of an agamic brood.

It follows from these observations, that the self-fertile Daphniæ are certainly true females, and that the reproductive bodies more nearly resemble eggs than gemmæ in their origin and development. Hereafter, however, it may be convenient to give a separate name to those egg-like bodies, which are fertile without impregnation, but for the present they must be called eggs.

The author then gives a list of the instances of Parthenogenesis which, so far as he knows, are recorded among the Articulata. Finally, he expresses the belief that the careful consideration of these cases, and of the facts now recorded as to *Daphnia*, and the still more wonderful observations recently detailed by Siebold in regard to *Apis* (if these latter are confirmed), must surely remove all lingering doubts as to the identity between eggs and buds; and remarks, that if Prof. Huxley's definition of "individual" and "zooid" is to be adopted, it will be impossible to assert of any Daphnia or Moth, whether it is the one or the other, and the hive-bee will have to be considered as an hermaphrodite, a species without male individuals.

Under these circumstances, the author suggests that it would be more convenient to continue, as heretofore, to call the individual of any species that which is individualized, even though in this case the individuals of one species will not always be homologous with those of another.

* It is worthy of notice, that the back fold indicating the divisions between the head and body is opposite the line between the mandibles and the first pair of maxillæ, which latter appear therefore to belong to the body, as Zaddach also asserts, and not to the head.

III. "On the Thermo-electricity of Ferruginous Metals, and on the Thermal Effects of stretching Solid Bodies." By J. P. JOULE, F.R.S. Received January 29, 1857.

The experiments on the above subjects were made with a thermo-multiplier placed in the vacuum of an air-pump. Its sensibility was such that with the junction antimony and bismuth, a thermometric effect not greater than $\frac{1}{8000}$ of a degree Centigrade could be estimated. In determining the thermo-electric position of the metals, it was necessary to increase the resistance of the instrument a hundred-fold, by placing in the circuit a coil of fine wire. In thermo-electric arrangement *steel* was found to be nearer copper than iron was. By hardening, steel was raised almost to the place of copper. *Cast iron* was found to surpass copper; so that the junction cast iron and copper is reverse to that of wrought iron and copper, and the arrangement cast iron and wrought iron is much more powerful than copper and wrought iron. A new test of the quality and purity of ferruginous metals is thus indicated, which will probably be found of value to the arts.

The experiments on the stretching of solids showed, in the case of the metals, a decrease of temperature when the stretching weight was applied, and a heating effect when the weight was removed. An iron wire $\frac{1}{4}$ of an inch in diameter was cooled $\frac{1}{8}$ of a degree Centigrade when stretched by a weight of 775 lbs. Similar results were obtained with cast iron, hard steel, copper, and lead. The thermal effects were in all these cases found to be almost identical with those deduced from Professor Thomson's theoretical investigation, the particular formula applicable to the case in question being $H=\frac{t}{J}\times Pe$, where H is the heat absorbed in a wire one foot long, t the absolute temperature, J the mechanical equivalent of the thermal unit, P the weight applied, and e the coefficient of expansion per 1°. With gutta-percha also a cooling effect on extension was observed; but a reverse action was discovered in the case of vulcanized india-rubber, which became *heated* when the weight is laid on, and *cooled* when the weight was removed. On learning this curious result, Professor Thomson, who had already intimated the probability of

a reverse action being observed under certain circumstances with india-rubber, suggested to the author experiments to ascertain whether vulcanized india-rubber stretched by a weight is shortened by increase of temperature. Accordingly, on trial, it was found that this material, when stretched by a weight capable of doubling its length, has that length diminished by one-tenth when its temperature is raised 50° Centigrade. This shortening effect was found to increase rapidly with the stretching weight employed; and, exactly according with the heating effects observed with different stretching weights, entirely to confirm the theory of Professor Thomson.

February 5, 1857.

The LORD WROTTESLEY, President, in the Chair.

The Bakerian Lecture was delivered by Michael Faraday, Esq., F.R.S., "On the Relations of Gold and other Metals to Light."

The Lecturer gave an exposition, illustrated by experiments, of the substance of a paper presented by him to the Society under the above title. The following is an abstract:—

The author of this paper hopes that the undulatory theory of light, when more fully and perfectly developed, may aid in comparing local actions with those which take place at a distance, and even help towards the comprehension of the physical means by which the latter are carried on; and with that view he endeavoured, experimentally, to subject a ray of light to the action of particles, so small in size as to have an immediate and near relation, not only to the undulations of light, but even to the far smaller motions of the parts of the ether, which are supposed to produce, by their joint and successive action, the light-wave. His hope was, that by choosing particles of a fitting substance, experimental results might be obtained which, in the hands of the mathematical philosopher, might aid in perfecting the theory; and for this purpose gold was selected, because of its high optical

qualities, shown in its comparative opacity, whilst possessing a real transparency; its high yellow reflexion and its true green transmission; its known action on light in very minute quantity; its capability of extreme division; its great gravitating force, which could be called upon for aid when the metal was in a state of extreme division; its elementary character; the integrity of its metallic state; the facilities of testing its presence and condition; and, finally, because known phenomena seemed already to indicate differences of action on light consequent upon its division.

The first state of division or attenuation considered, was that conferred on gold by beating into *leaves*. These, with their dimensions and general characters, are well known. Being taken up on glass damped by breathing or moistening, and then water introduced between the glass and the gold as a cushion, the gold can be perfectly stretched, so that when dry it is fit for optical examination; or if a diluted solution of cyanide of potassium be in like manner introduced beneath the gold, it can be more or less attenuated by solution, and then washed and dried. If gold-leaf thus extended and attached, either to glass, or plates of rock-crystal, or mica, be heated, it gradually loses its reflective power and its green colour, and becomes translucent. This change takes place far below the fusing-point of gold, and at a temperature as low as the boiling-point of oil if continued for several hours. When the heat is considerable, the gold-leaf suffers retraction of its parts, and becomes perforated by many fine holes, often symmetric in their form and dimension; but when the heat applied is the lowest competent to produce the change, it does not seem certain that the effect is due to such retraction; a good microscopic examination of this point is required. When pressure is applied to such decoloured gold by a convex piece of rock-crystal of short radius (as half an inch or less), the green colour of the transmitted ray reappears. This production of the green colour by pressure can often be referred to in different states of gold, as a proof, amongst others, that the metal is in the metallic condition. Silver-leaf undergoes a like change by heat, at even a lower temperature.

Division by the Leyden Deflagration.—When a gold wire is deflagrated near the surface of glass plates by a strong electric discharge, it is dissipated in minute particles, which are deposited on the glass. These are seen by the microscope to be of different sizes;

but by far the greater part are so minute as not to be distinguishable separately. The general film is of different colours by transmitted light, being grey, violet, or green; and often on the central or nearest part of the discharge, where the heat has been active, is of a fine ruby colour. All these particles act with acid and chemical reagents as gold acts; and there is no reason to believe they are anything other than metallic gold. They appear with precisely the same colours and characters, whether the deflagrations are made in common air, in oxygen, or in hydrogen; and whether the deposits are formed on glass, rock-crystal, topaz, or mica. When heated by any ordinary means, the green and grey parts change to a ruby or ruby-amethystine colour, and that whether surrounded by air, or vapour of alcohol or ether. Even after heating, they adhere only as a dust to the plates, except when the temperature applied to those on glass has been very high. Agate pressure confers the green character on the heated deposits, and also, in frequent cases, upon that which has not been heated. All things considered, there can be no reason to doubt that the deposits thus made to vary in the colour of the transmitted light, consist of pure metallic gold.

Thin films of Gold.—If a very weak solution of chloride of gold, free from excess of acid, and containing about 1½ grain of metal to 2 or 3 pints of water, be placed in a very clean glass or glazed vessel, in a quiet place, and then two or three small particles of phosphorus be laid floating on the surface, and the whole covered over and left for twelve or more hours, the gold will be reduced, covering the whole of the surface with a film, thicker near the phosphorus than at other parts. This film may be raised from the fluid by plates of glass, and washed and dried on the plates, and is then ready for examination. The thinner parts of such a film are scarcely visible, either by reflected or transmitted light; the transition to thicker parts is gradual, the thickest being opaque, and their reflexion that of dense gold. The colour by transmitted light varies, being grey, green, or dull violet. The films are porous, and act as pure gold, resisting all the agents which metallic gold resists. When heated, the transmitted colour changes towards amethyst and ruby; and then the effect of pressure in producing a green colour is in many cases very remarkable,—even a touch with a card or the finger being able to cause the change.

Gold fluids.—Whilst the particles of phosphorus are producing a film on the surface, it frequently happens that streams of a red colour descend from them through the fluid; and if the phosphorus be submerged, and left for twenty-four or forty-eight hours, this red product is easily and abundantly obtained. If the gold solution be placed in a very clean bottle, and then a few drops of a solution of phosphorus in ether be added, and the whole agitated from time to time, the ruby fluid is obtained in a shorter period. This fluid is apt to change in colour, becoming amethystine, violet, purple, and finally blue; impurities of certain kinds in very small quantities cause this change. It is hardly possible to clean a vessel so well that the first portion put into it does not alter. Most saline bodies produce the change; a trace of common salt readily makes it manifest. That all these fluids are coloured by diffused particles is shown by the circumstance, that on being left for a shorter or longer time, the particles sink, forming a coloured stratum of deposit; many months, however, are required for even the partial separation of the finer ruby particles. When a light is looked at through the fluid, the latter appears transparent; but when the eye is on the illuminated side, then the fluid is seen opalescent. If a cone of sun-rays be thrown by a lens into the fluid, the illumination of the particles within the cone shows their presence as undissolved bodies. It is believed that all the particles being metallic gold, the ruby are in the finest state of division, the blue in a more aggregated condition. Though the ruby particles, whilst freely diffused, are easily changed in colour, and as it is supposed by aggregation, still they may in some degree be separated by a filter; for on passing the fluid several times through a paper filter, the latter associates much of the rubifying substance with itself, and becomes of a rose colour; it may then be well washed and dried, and contains the ruby particles located, as it is believed, and prevented by their attachment to the paper-fibres from undergoing mutual aggregation. In this state their character is not altered from ruby to blue by salt or acids; they resist those chemical agents which are resisted by gold, but are dissolved by chlorine, cyanides and the other substances capable of acting on gold. Heated either in oxygen, hydrogen, or air, no change of tint or quality is induced at such temperatures as the paper can bear; or, as far as can be judged, at any higher temperature.

A ruby glass, coloured by gold, is well known. This is considered by the author as analogous to the ruby fluid just spoken of, being a diffusion of gold particles through vitreous matter. The ruby fluid by association with jelly is rendered much more permanent than before; and then it may by a little warmth be had in the fluid state, or by cooling as a tremulous jelly, or by desiccation as a hard ruby solid, presenting all the transitions between the gold fluid and the ruby glass. By soaking the dried jelly and then warming it with water, these transitions may be passed through in the reverse direction, and so on, any number of times.

The relations of gold (and other metals) to polarized light are of the following nature. A leaf of gold inclined at a certain angle across a ray of polarized light (the inclination not being in the plane of polarization or at right angles to it), affects it as a thin plate of any uncrystallized transparent substance would do, *i. e.* the light appears in the analyser, and the plane of polarization is rotated; or if a leaf of gold be held in an inclined position across a ray of unpolarized light, the beam is polarized as it would have been in passing through a like inclined plate of uncrystallized transparent matter. The gold greened by heating or pressure, when thus examined, does not appear to have acquired any particular tension or structure. Sulphide of carbon and crown-glass are optically so near each other, that a plate of the latter immersed in the former is neutralized; and though placed in an inclined position to a ray of light, either polarized or not, does not then affect it; but gold (and all metals) is still far above either of these. Hence the gold films obtained by phosphorus, when attached to glass, could be examined, and were found to have the optical properties of leaf-gold; the effect having no reference to the *thickness* of the film, but being most perfect in the thinner films because they were in a more regular and perfect condition. It should be remembered that these films are not continuous layers like coats of varnish or fluid, but easily pervious to vapours. In like manner the deposits of gold (and other metals) obtained by electric deflagrations, were examined and found to have the same marked qualities in a high degree; places where the film was scarcely visible on the glass, instantly showing the presence of the gold by their action on the polarized ray. In the same manner the very thin and almost invisible films, deposited occasionally on the sides of the vessels con-

taining the gold fluids, showed themselves as gold. The thinnest layer of the fluid itself, however rich in particles, held between two plates of glass, acted no otherwise than a layer of water. It appears by the deflagrations that the particles of gold must be deposited in a plane, and then, though discontinuous, they act in the manner of continuous films of ordinary uncrystallized transparent bodies.

As to the quantity of gold in the different films or solutions, it can at present only be said that it is very small. Suppose that a leaf of gold, which weighs about 0·2 of a grain, and will cover a base of nearly 10 square inches, were diffused through a column having that base, and 2·7 inches in height, it would give a ruby fluid equal in depth of tint to a good red rose; the volume of the gold present being about the $\frac{1}{500,000}$th part of the volume of the fluid; another result gave 0·01 of a grain of gold in a cubic inch of fluid. These fine diffused particles have not as yet been distinguished by any microscopic power applied to them.

February 12, 1857.

W. R. GROVE, Esq., V.P., in the Chair.

The following communications were read:—

I. "On the Photography of the Moon." By WILLIAM CROOKES, Esq. Communicated by Professor STOKES, Sec. R.S. Received December 16, 1856.

The subject of lunar photography is one which has engaged the attention of scientific men almost from the first announcement of the possibility of fixing the images in the camera. Owing to the extreme difficulty of satisfying all the conditions of the problem to be solved, there are few good photographs of the moon yet in existence. It was my good fortune in the autumn of 1855 to obtain several excellent pictures of this kind, and since these form the starting-point of the work which, by the assistance of a grant from the Donation Fund of the Royal Society, I have been pursuing during the greater part of the last year, a detailed account of the means employed for their production will not, I think, be considered out of place here.

The telescope in which these pictures were taken is the magnificent equatorial at the Liverpool Observatory. This, together with all the resources of the establishment, was placed at my disposal by my kind friend Mr. Hartnup, to whom it is but due to state, that, were it not for the invaluable assistance afforded me by his sterling advice as well as steady hand, the results would not have been worth keeping.

The mounting of the equatorial is quite unique; the polar axis and telescope together weigh about five tons, and whilst all parts are so truly and smoothly fitted that this enormous mass is moved equatorially by means of a small water-mill with such marvellous accuracy,

that a star viewed through it appears absolutely stationary, its firmness is such that a hard blow against the side merely produces a scarcely perceptible momentary deflection. The object-glass is 8 inches in diameter, and has a sidereal focus of 12·5 feet—the diameter of the moon's image in this focus being about 1·35 inch.

The eyepiece was removed, and in its place the body of a small camera was attached, so that the moon's image might fall upon the ground glass or sensitive film in the usual manner. Much labour had been saved me in finding the true actinic focus, by several photographers of Liverpool, who were working for some time on the same subject when the British Association met in that city in 1854. They found that the object-glass had been over-corrected for the actinic rays—the plate being required to be placed at a distance of 0·8 of an inch beyond the optical focus: a few experiments were sufficient to enable me to verify this result.

During the time above referred to, and frequently since, Mr. Hartnup had taken many hundreds of pictures with chemicals recommended by various persons, but had not succeeded in obtaining a good negative at all, and not even a positive with a less exposure than from half a minute to a minute. As I succeeded in taking dense negatives in about four seconds, with the temperature of the room below freezing and the moon at a considerable distance from the meridian, and as I attribute the greater sensitiveness which I obtained to the great purity of the materials I employed, I think it right to give, after the mechanical arrangements are described, an account of the way in which these were prepared.

The clockwork movement was only sufficient to follow the moon approximately when on the meridian, but as the pictures were nearly all taken when she was some distance past the meridian, and when consequently the declination and atmospheric refraction were changing rapidly, it was necessary, notwithstanding the short time required to take the pictures, to correct for the imperfect motion of the telescope. This was done by means of slow-motion screws attached to the right ascension and declination circles, which are each 4 feet in diameter. The finder had an eyepiece with a power of 200 applied to it, having cross wires in its field.

The *modus operandi* of taking the picture was as follows:—The telescope having been moved until the moon's image was in the centre

of the focusing glass, the water-mill was turned on and the dark slide containing the sensitive collodion plate was substituted for the ground glass. Mr. Hartnup then took his station at the finder, and, with a tangent rod in each hand, by a steady and continuous movement, kept the point of intersection of the cross wires stationary on one spot on the moon's surface.

When the motion was most perfectly neutralized, I uncovered the sensitive plate at a given signal and exposed it, counting the seconds by means of a loud-ticking chronometer by my side.

From the ease with which on my first attempt I could keep the cross wires of the finder fixed on one point of the moon by means of the tangent rods, I confidently believe that with the well-tutored hands and consummate skill which guided this noble instrument, the moon's image was as motionless on the collodion plate as it could have been were it a terrestrial object.

The negatives which I obtained by these means were exquisitely beautiful, and so minute that I could not obtain paper with a sufficiently fine surface whereon to print copies which would do them justice. It was evident that they would bear magnifying several diameters and still remain sharply defined. The expense of carrying out this design here stopped me, when by the kind advice of Professor Wheatstone I applied to the Royal Society, whose munificence has so frequently been the cause of bringing to a successful termination investigations of the highest importance.

A half-plate photographic combination of lenses, by Ross, was screwed the reverse way into a large sliding camera body 10 inches high by 11 inches wide, and capable of sliding from 18 inches to 3 feet long. At the end of the sliding body opposite to the lens, was a groove to admit either a focusing glass or a dark slide for the sensitive plate. A smaller camera body was screwed into the other end of the brasswork of the lens, having also a groove in front to admit of a sliding box capable of holding the small negatives. A reflector was placed in front of all, so arranged as to move in altitude round a centre, and, being fixed in any required position, to reflect the diffused light of the sky through the negative and lens parallel with the axis of the latter.

Preliminary trials showed me that there was no good gained by magnifying the small pictures more than about 20 times, as after

that the individual parts begin to get confused and indistinct; this magnifying cannot, however, be effected at once. In the small negatives the lights and shades are the reverse of what they are in nature, consequently a print on paper therefrom gives the light and shade correct. A photographic copy of a negative, however, produces a positive by transmitted light, and a print from this would have the shadows light and the light parts dark; consequently, in magnifying a negative with the intention of still producing a negative, an intermediate transmitted positive must first be taken, and this in its turn magnified, when it will produce a negative.

The relative distances of the negative and focusing glass from the intermediate lens were so adjusted, that an image of the negative, enlarged to about two diameters, was thrown upon the ground glass, care being taken that the light from the sky was reflected parallel through the centres of the negative and lens by means of the mirror. The aperture of the lens was then stopped down to half an inch by means of a diaphragm, and the focus most carefully obtained by sliding the end of the large camera in or out. I found it necessary to verify this by experimental trials at different distances on each side of the observed focus, as it was difficult to judge accurately with the eye on the ground glass, owing to the roughness of the latter and the feebleness of the light.

A picture, or rather many pictures, were now taken, and the one which by transmitted light most truthfully resembled a paper print from the small negative was reserved for further magnifying. This was effected absolutely in the same manner as the former: the negative being removed and the positive being placed in its stead, a further magnifying gave a large-sized negative.

Although this process seems very simple, it is impossible to estimate the difficulties, unless by an actual repetition of the experiment, which I had to overcome before arriving at the beautiful result which I have the honour to lay before the Society. The double copying had a tendency to slightly exaggerate the effect of light and shade, and this could only be obviated by exposing the plates for such a time, that with the feeble light at my command it was verging on decomposition; particles of dust, too, seemed most pertinaciously to fix themselves on the prominent mountains, giving rise to craters where none should be; and even my finished pictures are not per-

fectly free from these faults, although each negative is the representative of a month's work and upwards of a hundred failures.

I doubt if much better photographs of our satellite can be taken by the way I have pursued. The future of lunar photography lies in another direction: the image must not be received on a sensitive plate and this copy submitted to an after process of magnifying. Defects quite imperceptible to the naked eye on the small negatives, are expanded into great blotches when magnified. In fact, upwards of a dozen seemingly equally good negatives with which I started, have, with but one or two exceptions, shown spots when enlarged.

The magnifying must be conducted simultaneously with the photographing, either by having the eyepiece on the telescope, or better still, by having a proper arrangement of lenses to throw a magnified moon image at once on the collodion. The difficulty of want of light could not be any objection, as supposing the enlarged image to be equal to those which I have now taken, that would be an increase of area of about twenty times, consequently 20×6 seconds, or 2 minutes, would represent the average time of exposure; a period which, even were it prolonged four or five times, would not then be too severe a tax upon a steady and skilful hand and eye.

Description of the Photographic Process.

The glass employed for taking the original negative of the moon, was that known as "extra white colour patent plate," that for the intermediate positives and large negatives was ordinary patent plate. Cleaning the surface, which is an operation of especial importance, was effected in the following manner.

The glasses were dipped into and then well rubbed over with a hot solution of caustic potassa; then, after washing with water, they were transferred to hot nitric acid (one part strong acid to three of water), where they were allowed to remain for about half an hour.

A piece of soft wash-leather was plentifully rinsed, first in a warm dilute solution of carbonate of soda, afterwards in clean water, and then well wrung until all the superfluous water was squeezed out. The glass plates were taken from the nitric acid and rinsed in abundance of clean water, and then rubbed well on every part with the damp leather. This removed most of the superficial moisture; and the final drying was effected by means of another piece of wash-

leather, prepared the same as before, but allowed to become perfectly dry.

Just previous to using, the plates, held in a pneumatic plate-holder, had the last polish given to them by briskly rubbing with a warm piece of fine diaper (which had also been previously washed in soda and water, and then well rinsed and dried) until the moisture condensed from the breath evaporated evenly and uniformly, especially guarding against the slightest contact between the surface of the glass and the fingers.

The plate was now held with its clean side downward until the collodion was about to be poured on, and every particle of dust (which was easily seen by bringing the source of light, the under surface of the plate and the eye, nearly in the same line) was gently wiped off by passing a warm piece of fine cambric lightly across.

Care was also taken to have the atmosphere of the room as free as possible from floating particles, and the dried collodion usually adhering to the neck of the bottle was scrupulously removed.

The collodion was poured on and the plate rendered sensitive in the usual way. As the temperature both of the equatorial and operating rooms was seldom far from the freezing-point, the great diminution of sensitiveness, which that circumstance would have occasioned, was obviated by having the nitrate of silver bath and developing solution warmed to about 30° C., and also by slightly warming the plates before using. The source of light was a fishtail gas burner in the outer room, and shining close to the orange glass window of the dark room.

The soluble paper for the collodion was prepared in the following manner:—a mixture was made of

	sp. gr.	
Commercial nitrous acid	1·43	4 fluid ounces.
Commercial nitric acid........	1·37	4 „
Sulphuric acid	1·82	8 „

When the temperature of the mixture had cooled down to 50° C., one sheet of Swedish filtering-paper, torn up into small pieces, was completely immersed in the mixture, and allowed to remain therein for about half an hour. It was then thrown into a large pail of water, and the paper removed and placed on a sieve under a running tap for a quarter of an hour; after washing in very dilute solution of

ammonia and then in plenty of water, the paper was pressed between the folds of a cloth, and then allowed to dry spontaneously in the air.

The collodion was made with—

Ether, sp. gr. ·725 (previously freed from acid by rectification from dry caustic potassa)	5 fluid ounces.
Absolute alcohol	3 „
Soluble paper (dried at 100° C.)	50 grains.
Iodide of cadmium (*pure*)........	30 „

The alcohol and ether were mixed together, and then the paper and iodide of cadmium were added: they dissolved in a few minutes with a little shaking. As soon as the solution was complete, it was allowed to stand for twenty-four hours, and then half of the clear supernatant fluid was decanted carefully into a clean well-stoppered bottle for use. I believe that collodion prepared in this way will remain uniform from one year's end to another.

The nitrate of silver bath was made by dissolving 1 ounce of crystallized nitrate of silver, perfectly pure and neutral, in 2 ounces of water, then, with constant stirring, adding a solution of 4 grains of iodide of cadmium in 1 ounce of water, and a quarter of an ounce of the above iodized collodion, and water to make up the volume to 10 ounces. This was allowed to stand for a few hours at a temperature of about 25° C., and then filtered from the undissolved iodide of silver and precipitated paper. A glass bath was used in preference to gutta percha, and, as above stated, it was heated to 30° C. when used.

The developing solution consisted of—

Pure pyrogallic acid	8 grains.
Crystallized citric acid	16 „
Water..........................	8 fluid ounces.
Alcohol	½ „

This developing solution is very slow in its action, 15 to 20 minutes being frequently required, but it ultimately produces negatives of such vigour and freedom from stains, that I much prefer it to the usual formula.

The fixing solution employed was the ordinary nearly saturated solution of hyposulphite of soda. After its employment the pictures

were well and carefully washed in warm water, dried before a fire, and, after scratching the description or name on a corner, varnished with the usual solution of amber in chloroform.

The subsequent operation of printing is so easily performed, and has been so fully described by persons of more experience than myself, that any further allusion to it will be needless.

Appendix.

Besides the pictures taken in America—which are almost valueless as moon maps, as the sides are reversed in the copying from the daguerreotype plate upon which they were originally taken,—the moon has been photographed by Professor Phillips, Father Secchi, MM. Bertsch and Arnauld, several Liverpool photographers, and Mr. Hartnup and myself. It is interesting and instructive to compare among themselves the means employed and the time occupied in taking the impression on these several occasions.

Professor Phillips's telescope has a sidereal focus of 11 feet, and an aperture of $6\frac{1}{4}$ inches; consequently the brilliancy of the moon's image in its focus is augmented 26 times over what she appears to the naked eye. The average time occupied for the collodion plate to receive the impression was about 3 minutes.

Father Secchi's telescope having a sidereal focus of 18 times its aperture, the moon's image was intensified 37·8 times, and the time required for the impression was an average of 6 minutes.

M. Porro's glass of 49 feet sidereal focus and 20 inches aperture, gave a moon image 12·3 times brighter than she appeared to the naked eye, and the average time of taking the picture was 17 seconds.

Mr. Hartnup's telescope being $12\frac{1}{2}$ feet focus and 8 inches aperture, augments the intensity of the moon's image at its focus 35·1 times. The time which was required for the photograph of our satellite to be taken, on the occasion of the meeting of the British Association at Liverpool in 1854, was about 2 minutes; and under the same circumstances we ourselves succeeded in obtaining perfect and intense negatives in 4 seconds. These, however, were taken under very unfavourable circumstances, the temperature being below the freezing-point, and the moon at a considerable distance from the meridian,

which necessarily caused both a diminution of the light and also a diminished sensitiveness of the collodion film.

The rapidity with which the above pictures were taken may be better understood by comparing them with those of terrestrial objects under similar circumstances. According to Herschel*—

"The actual illumination of the lunar surface is not much superior to that of weathered sandstone rock in full sunshine. I have frequently compared the moon setting behind the grey perpendicular façade of the Table Mountain, illuminated by the sun just risen in the opposite quarter of the horizon, when it has been scarcely distinguishable in brightness from the rock in contact with it. The sun and moon being nearly at equal altitudes, and the atmosphere perfectly free from cloud or vapour, its effect is alike on both luminaries."

Thus by comparing the Liverpool object-glass as to power with our ordinary camera lens, its focal length being nearly 19 times the aperture, and the moon's image being copied by its means in 4 seconds, we find that it is equivalent to copying sandstone illuminated by the sun in 4 seconds with a lens $4\frac{1}{2}$ inches focus and a little less than $\frac{1}{4}$ inch diaphragm; or with a compound lens having an aperture of one inch, and the same focal length, in a quarter of a second.

II. "Researches on the Reproductive Organs of the Annelids." By Thomas Williams, M.D., F.L.S., Physician to the Swansea Infirmary. Communicated by Thomas Bell, Esq., F.R.S., P.L.S. &c. Received December 30, 1856.

(Abstract.)

In this paper the author seeks to establish the following general proposition, viz. that there prevails throughout the Actiniadæ, Echinodermata, Rotifera and Annelida, a special organ, which, under different phases, subserves different functions, which is essentially *identifiable* under every modification, reducible to the same type, and which constitutes the *root* of the Reproductive system in these families. To this special organ he proposes to apply the provisional

* Herschel's Outlines of Astronomy, page 249.

name of the "*segmental organ**." In the chambers which are enclosed by the vertical dissepiments dividing the body of *Actinia*, convoluted tubular cords are contained which support the generative structures. It has not yet been proved whether the internal ends of these tubules open directly into the perivisceral chamber. These cords and their appended structures in the Actiniadæ, constitute the *type* of a system of organs the prevalence of which throughout the Echinodermata, Rotifera and Annelida can, he believes, be clearly and satisfactorily proved. In the present memoir, however, the author proposes to confine his demonstrations to the anatomical varieties which the segmental organ presents in the class *Annelida*, contenting himself with merely in a passing manner pointing out the fact that the several variations of form, structure and number which this organ exhibits in the several genera of this class, are *represented* by similar variations in the different genera, especially of the class Echinodermata. He hoped to show that under very numerous apparent varieties, the essential unity of the segmental organ within the indicated limits can be convincingly established.

Upon this organ, under different circumstances, there devolve one or two or even more functions. Sometimes it is used as a simple *discharge tube*, conveying externally in a direct manner the fluid of the general cavity of the body. This variety is exemplified in the segmental organs which are distributed, in the genera *Lumbricus* and *Nais*, throughout all that part of the body which is situated posterior to the Reproductive band. In this latter region two or more of these organs are so modified as to become the basis whereon is developed the generative structures.

Here the author enters upon a minute account (illustrated by figures) of the history of this organ in *Lumbricus* and *Nais*, showing the changes of outward form which it undergoes in several species of these genera.

He points out in this place that the segmental organ, as it occurs in *Lumbricus* and *Nais*, is paralleled by the so-called water-vascular system of the Rotifera: as in the former so in the latter, the ciliated tubes communicate openly with the general cavity; in both, the cur-

* While he is convinced that the identity of this organ might readily be traced throughout other families of the lower Invertebrata, he will not permit himself at present to indulge in any wider generalization than that stated in the text.

rent raised by the cilia travels from within outwards; and he contends that the reproductive structures are ingrafted upon, or developed from one, two or more of the ciliated tubes in the Rotifera, as from the segmental organs of *Lumbricus* and *Nais*.

Arenicola and *Terebella* form a group in which the segmental organ deviates in a remarkable apparent manner from that of *Lumbricus* and *Nais*. It forms a series of elongated sacculi, which are attached to the ventral wall of the general cavity on either side of the median line. Each sacculus, although single at its distal end, is divided at its attached end into two tubular limbs, one of which communicates directly with the exterior, while the other opens immediately into the general cavity of the body. Through the latter limb the *ova* and *sperm-cells* are introduced into the perivisceral chamber, while in the reverse direction the *fluid* of this chamber is discharged externally. The author has never been able to discover how the germ- and sperm-cells (respectively in the female and male) escape out of the general cavity. But he trusts that he has given a new and satisfactory demonstration of the mode in which they *enter* that cavity. The genera *Arenicola* and *Terebella* comprehend the *only Annelids* in which the germ-elements in the female, and the sperm-cells in the male, are ushered into, and are required to sojourn for a season in the fluid of the general cavity of the body.

He indicates in this place that the segmental organ of the *Sipunculidæ* (amongst the Echinoderms) corresponds both in its structure and relations to that of *Arenicola* and *Terebella*, with this difference only, that in the latter a special and *peculiar development* of the blood-vascular system occurs around and in the vicinity of the segmental organ, whereas in the Sipunculidæ this system scarcely exists and never receives any enlargement. The segmental organs in the genus *Synapta* stand in an intermediate position between those of *Holothuria* and those of *Sipunculus*. In *Synapta* one or more organs remain in the condition of simple 'discharge tubes,' while others become developed into the Reproductive structures.

The segmental organs of the Hirudinaceæ are next described. The author adheres almost in every detail to the results published by him in 1851 in the Transactions of the British Association, with reference to the reproductive system of this family of *Annelids*. In the present memoir he records the results of new and carefully conducted dissections, which prove that in the Common Leech, the Sea-leech,

and probably in the genus *Clepsina,* there is situated an organ on either side of the ventral median line, which is repeated *in every ring of the body,* and which in this family is the true ovigerous apparatus, the testes constituting a separate and more medianly disposed series of glandular bodies, whose homologies he has not yet satisfactorily determined.

The so-called "respiratory sacculus" of Dugès he now looks upon as the process of the ovario-segmental organ, by which a communication is established between the latter and the general cavity of the body, and by which the fluid of this chamber escapes externally. All the Hirudinaceæ are androgynous.

Under the Nereid group is included in this memoir, the genera *Nereis, Aricia, Phyllodoce, Nephthys, Syllis* and *Nerine.* The segmental organ in these families is specially described and figured. In all, the sexes are seated on separate individuals. In no single instance is the general cavity rendered subservient to the incubatory process. In all, the general circumference of the organ is lobulated and irregular, entering the hollow bases of the cirrhi and blended most intimately with the blood-vascular-system.

Glycera and *Cirrhatulus* the author classes together, on account partly of the similarity of form and structure of the segmental organ, but especially because in both the *blood-vascular system* is *completely and entirely wanting,* its absence being compensated by the existence of a second order of pigment-carrying corpuscles in the cavitary fluid. These genera are unisexual, and at no time are the germ- and sperm-elements introduced into the perivisceral chamber.

In this and the preceding groups the author has not succeeded in discovering the mode in which the segmental organ opens into the general cavity; but from the fact that it has a looped arrangement, supported on two tubular limbs, he is quite convinced that an opening into this cavity, for the purpose of giving direct outlet to its contents, does really exist. This conclusion is fortified by the analogy of the form under which the organ exists, in the Nereid group in general.

The *Nemertine Annelids* are then examined. The author recalls the results of his researches as published in his 'Report' on the Annelids in 1851. His renewed investigations have confirmed the statements which he then put forth. He still contends that what M. Quatrefages has described in these worms as the *ovary* is a great

alimentary cæcum, and that the Reproductive system consists in a double series of segmental organs, one on either side of this great cæcum and the ventral median line; that in this family the sexes are seated on separate individuals; that the reproductive elements at no time find their way into the general cavity; that in fact these Annelids, anomalous only in the disposition of their alimentary system, conform, as regards the type and mode of repetition of segmental organs, to the standard offered by the great Nereid group.

Chloræma Dujardinii, especially the female, presents an extremely favourable opportunity of observing the *looped arrangement* of the segmental organ, and of proving the fact, so constantly seen in the other families of Annelids, viz. that *one limb* of the loop is the true ovary, or the primary seat of the ovo-genesis; that the ovules travel round the curve of the loop; that they acquire a considerably developed size at the other limb, just before they escape externally. In *Chloræma* the vitelline body of the ova is almost ink-black. The entire extent of the segmental organ is thus rendered perfectly and easily traceable amid the surrounding transparent structures. In this genus it does not at present appear that either limb of the loop opens into the general cavity. This type prevails, as far as he knows at present, throughout the Nereid group; in other words, in these Annelids the function of a *discharge tube* does not appear to be thrown upon the segmental organ under any circumstances; hence the limited dimensions of the general cavity and the highly developed character of the blood-vascular apparatus.

The group lastly examined, is that of the Aphroditaceæ. On the reproductive organs of this large, numerous and interesting family, no single observation is contained in any work on comparative anatomy that the author is acquainted with. He enters upon a detailed account of his own researches. They have ended in what he would fain believe to be a complete solution of the difficulties in which the history of the Reproductive or segmental system of this family has been hitherto shrouded. This family, without exception, is unisexual. The segmental organs constitute a complete and regularly branched series, situated, as in all other Annelids, on either side of the ventral median line. They embrace, like a gauze-work, the diverticula of the alimentary system, with which they correspond in number. They fall under the general designation of the "segmental

organ," in the feature of their having *two limbs*, being therefore looped, although only rudimentarily. The author then proceeds to give an account of his studies into the history of this organ amongst the grotesque and highly varied species of the genus *Polynoë*. They have rendered it certain that throughout the family of the Aphroditaceæ there obtains but *one type of segmental organ*, and *that upon it always are ingrafted the generative structures.*

At this stage the attempt is made to show that the segmental organs of the Echinidæ, Asteriadæ and Holothuriadæ conform, structurally and functionally, in the most remarkably intimate manner with the typical standard exhibited by this organ in the Aphroditaceæ. But between these Echinoderm and Annelidan families the author attempts to indicate other zoological affinities. He shows, that, according to his researches amongst the Aphroditaceæ, there is no trace whatever to be discovered of a blood-vascular system. In this respect they correspond with the Echinidan and Asteridan families. He shows that in the Aphroditaceæ the general cavity is never, under any circumstances, used as an incubatory chamber. In this point of their generative history the Echinidæ and Asteriadæ exactly agree with the Aphroditaceæ.

The author regrets, that, in consequence of the difficulty of obtaining specimens, he is obliged to defer to a second memoir many special points of anatomical structure and physiological relations, the determination of which he still feels to be necessary to the complete history of the segmental organ in the Annulose and Radiated classes.

The paper is illustrated by numerous drawings.

III. "Addition to a Memoir on the Determination of Unknown Functions that are evolved under Definite Integrals." By J. Gomes de Souza, Esq. Communicated by Dr. Sharpey, Sec. R.S. Received November 1, 1856.

In his previous communication (Proceedings, June 12, 1856) the author developed ϕx in terms of the function A $e^{m_r x}$. In the present communication he developes in a more general way, using terms of the form $A_r \int_r^\delta e^{x\omega} \varpi(\omega_1 m_r) d\omega$, the function of ϖ being assumed at pleasure.

February 19, 1857.

Dr. W. A. MILLER, V.P., in the Chair.

The following communications were read:—

I. "History of two Cases of Hernia of the Ovaries, in one of which there was a periodical Enlargement of one or other of these Organs." By HENRY OLDHAM, M.D., Member of the Royal College of Physicians, and Obstetric Physician to Guy's Hospital. Communicated by ARTHUR FARRE, M.D., F.R.S. Received January 10, 1857.

The two cases which I beg to present to the Royal Society, are examples of a rare conformation of the female sexual organs, in which the two ovaria have descended through the inguinal canals and have become permanently lodged in the upper part of the external labia; and in both of them the most careful and repeated physical examination has failed to detect either uterus or vagina. I should hardly, however, have ventured to bring them before the Society simply as examples of faulty conformation—because, although rare, yet they have been observed*—had not the first of the two cases presented the interesting physiological peculiarity of a spontaneous periodical increase of one or other of the ovaria, followed by its gradual reduction, thus supplying direct evidence of an ovarian menstrual act.

The subject of the first case applied to me in September 1851 for advice on account of never having menstruated. She was nineteen years of age, of a tall figure, symmetrical frame, well-expanded pelvis, and womanly aspect, bearing all the marks of a full completion of the physical changes of puberty; and her general health, though not robust, was fairly good. She was one of a family of five children, and her sisters had menstruated between fifteen and sixteen years of

* *Vide* Deneux, "Recherches sur la Hernie de l'Ovaire," 1813.

age. The principal point which was elicited from her history in connexion with her complaint was, that, eighteen months before, a swelling had somewhat suddenly appeared on the right side of the external organs, which had caused her some pain for a few days, and had then disappeared. In four or five months a similar swelling again appeared, but was attended with so much suffering that a medical man was consulted, who took it for an abscess and ordered it to be poulticed. Again it passed away, again to recur at the end of two months; and so it had gone on at irregular intervals until the time of her seeing me, when the pain of a renewed attack of unusual severity had occasioned her some alarm. On examination, a swelling the size of a goose-egg was found to extend between the external abdominal ring and the centre of the labium on the right side, which was very tense and firm to the touch; and the cellular tissue, skin, and mucous membrane of the labium were œdematous and inflamed. It was painful, but by no means so painful as a labial abscess, which in its general aspect it resembled; and there was but little febrile disturbance. A more critical examination detected the presence of a solid body of an oval shape within the tissue of the labium, which proved to be the ovarium, whose enlargement had so compressed the surrounding tissues as to swell and inflame them. On the opposite side there was another oval body, the size of a walnut, which passed just beyond the outer ring, but readily slipped into the canal. This was the left ovary in a quiescent state. The external sexual parts were normally formed; but the ostium vaginæ was closed, a slight indentation in the median line alone marking its position. Frequent careful physical examinations failed to detect any trace of a vagina or uterus, and the conclusion arrived at was that these central pelvic organs had not been developed. The mammary glands were fully formed.

I have had repeated opportunities during the six years which have intervened since first the case came before me, of examining the organs both during the periods of ovarian excitement and during the intervals. For nearly two years, however, I completely lost sight of her, when I learned to my amazement, that, in spite of my strong admonition both to her mother and herself that she should lead a single life, she had married. For some time past I have seen her more frequently, and have watched the recurrence of the ovarian swellings.

For the first three years the right ovarium was exclusively enlarged, and the intervals were not so regularly marked, varying between three and six weeks; excepting for the first year, when they were much longer, occasionally extending to three months. For the last two years the left ovarium has been far more frequently affected, the right remaining quiescent; occasionally both are painful and tumid, but even then one more than the other. The intervals are now pretty regularly three weeks. The acute inflammatory symptoms which accompanied the onset of these swellings have long since ceased to recur, which is obviously due to the loose state of the tissues from repeated stretching, so that the swollen organ is no longer compressed.

The accession of a menstrual time is sometimes suddenly felt. She will go to bed well, and in the morning the ovary will be swollen: more commonly, however, it is very gradual, augmenting in volume for four days, then remaining stationary for three days, and then gradually declining; the whole process, before the ovary is reduced, generally lasting ten or twelve days. On separating the ovary, when at its height of swelling, from the tissues surrounding it, it appears scarcely, if at all, less than double its usual volume; its outline is clearly defined, and it is plain that the whole, and not merely a part, of the organ is involved. There is no suffering worthy of notice during the time: the swelling is tender if pressed; and tender, too, in the act of sitting down or rising up; but she walks about as usual without distress, and there is but little lumbar or hypogastric pain. Neither are there any manifest sympathies excited, either of the mammary glands or other organs. Nor is there any vicarious flux, either of blood or any secretion, with the exception of an excess of saliva, but this not in any large flow. The ovary alone appeared to be engaged in this periodical act, which it is not too much to suppose, in accordance with modern physiological views, would have been attended with a flux of blood, had not the organs which normally supply it been absent.

But while this may be said to represent the usual course of a period, yet the volume of the ovary, and the length of time it remains swollen, is subject to occasional variation; sometimes being much less tumid, and dying away in a shorter time.

The repeated attempts at sexual union have only had the effect of somewhat loosening the tissues around the vulva, but the vagina

remains imperforate as before, and is beyond the reach of surgical remedy. It may be added, that the subject of this history recognizes an increase of sexual feeling at and soon after the periods of enlargement of the ovary.

The second case was that of a young woman who had attained the age of twenty without having menstruated. She was a tall, strumous-looking person, in weak health. There had not been any well-marked efforts at menstruation, but she had suffered slightly from lumbar pain. The mammæ were well developed. The pelvis was fairly formed. On examination I found the two ovaria just appearing beyond the external abdominal rings, and readily returning by pressure into their respective inguinal canals. They were of equal size and similar shape, being ovoid bodies about the size of small chestnuts. They were not tender when touched, although organically sensitive, and she had never experienced pain in them. The external sexual organs were somewhat less perfectly developed than usual; the vaginal orifice was closed, and no trace of a canal or uterus could be detected by exploration with a catheter in the bladder and the finger in the rectum. These organs, as in the former case, were absent. During the time I saw the patient, which was only for two months, the ovaria did not enlarge, although her general health improved.

II. "Further Observations on the Anatomy and Physiology of *Nautilus*." By John D. Macdonald, Esq., Assistant Surgeon R.N. Communicated by Captain Denham, R.N., F.R.S. Received January 13, 1857.

Both Professors Owen and Valenciennes noticed that the hollow subocular process of their specimens of *Nautilus Pompilius* was not tentaculiferous, and I may be permitted to say that this was also true of several examples of *Nautilus Pompilius*, and one of *N. macromphalus*, examined by me. But there is still another matter worthy of remark with reference to this process, namely, that its cavity may be traced downwards, inwards, and a little forwards, to within about the twentieth of an inch of the auditory capsule; indeed it would

appear as though provision had been made for the entrance of sonorous waves through a rudimentary external ear.

There can be little doubt that the eye itself is a modified tentacular sheath, so fashioned and endowed as to become the seat of the special sense of vision; but the subserviency of such a part to the faculty of hearing is much more obviously seen in the subocular process just noticed, which holds an intermediate position between the organ of vision and the tentaculiferous sheaths protecting the proper organs of touch.

In a figure which accompanies this communication, the auditory sac is exposed by an incision made in the groove between the funnel-lobe and the base of the tentacular sheaths. The subocular process is slit open to the bottom of its cavity, so as to show its termination in close proximity to the ear-sac. The interior of the tube is lined with a glandular membrane thrown into small folds, disposed longitudinally, but the exterior of the process is quite smooth like the rest of the integument.

I have often had some little difficulty in detecting the otolithes or otoconia, as the case may have been, in gasteropods long immersed in spirits or other preservative fluids; but in a specimen of *N. Pompilius*, kept for many months in strong gin, although the soft parts were far from being well preserved, I was enabled at the first attempt to remove the contents of the auditory sacs, and the minute elliptical otoconial particles, identical in character with those of *N. macromphalus*, were very distinctly seen under the microscope.

In a former paper, I first noticed my discovery of simple auditory capsules in, as I then supposed, the *N. umbilicatus*; but I find that I have incorrectly named my specimen, for on comparing the shell with the drawings of the several existing *Nautili* given in Sowerby's 'Thesaurus Conchyliorum,' it agreed exactly with the figure of *N. macromphalus*. I am indebted to my friend Mr. S. Stutchbury for the perusal of the work referred to, and my error is sufficiently accounted for by the scantiness of my own library.

With reference to the action of the great lateral muscles of *Nautilus*, the following ideas have suggested themselves to my mind.

As though preparatory to the complete separation of the body of the Cephalopod from the shell, which is usually present in the lower genera, the fasciculi composing the lateral muscles in *Nautilus* do not

perforate the mantle, and therefore cannot be directly fixed into the shell; they are, however, connected with it through the medium of thin filmy layers of a corneous texture, which frequently remain attached to the shell after the animal has been removed. The feeble hold of those muscles, even in a very recent state, is thus readily accounted for. Indeed, it is highly probable that the fixity of the body of *Nautilus* during the inhalation and forcible ejection of the respiratory currents is effected by the shell-muscles reacting upon one another, on the principle of a spring purchase, rather than by simple traction, as illustrated by the withdrawal of a gasteropod within its retreat, or the closure of the valves of a conchifer by the adductor muscles.

This view, which is supported by the foregoing facts, has its principal basis in the line of direction of the shell-muscles, and the angle at which they meet one another, at the root of the funnel-lobe; for the outer extremity of each being fixed, it follows that the first effect of the contraction of the muscular fibres would be to increase the angle just noticed; and this cannot possibly be accomplished, according to the recognized laws of muscular action, without tending to throw apart the points of origin, or in other words, exerting outward pressure against the internal wall of the shell, and thus, as it were, jamming the occupant tightly in its cell.

The action of the great lateral muscles of *Nautilus* here supposed, affords a remarkable contrast with the mode in which the posterior expanded arms of Argonauta embrace the exterior of its shell, particularly during the ejection of the expiratory current; while the withdrawal of the gasteropod into its abode, by the contraction of a veritable retractor, exhibits the exertion of muscular force in a very different direction.

In regard to the supposition that *Nautilus macromphalus* is the male of *N. Pompilius*, I may remark, that, besides my own specimen of the former, which proved to be a female, another, in very excellent condition, lately deposited in the Sydney Museum, is of the same sex.

III. "Brief Description of a Ctenostomatous Polyzoon, allied to Vesicularia, occurring on the Australian Coast." By JOHN DENIS MACDONALD, Esq., Assistant Surgeon R.N. Communicated by Captain DENHAM, R.N., F.R.S. Received January 13, 1857.

In one of our visits to Moreton Bay, the sean was hauled at Moreton Island, and amongst the masses of sea-weed, &c. brought up with the net, I found numerous specimens of a very beautiful Polyzoon, a small portion of which I had previously dredged at Port Stevens from a depth of 5 or 6 fathoms.

The Polypidom may be said to have consisted mainly of rooted, spreading and plantlike portions, and short, straight creeping trunks, connected at both extremities with the fixed part of the former, so that the whole presented the appearance of an open lace-work, having all the transparency and lustre of glass.

The trunks and branches were nearly perfectly cylindrical, and composed of an outer membranous sheath distended with a clear fluid (which escaped with considerable force when the sheath was ruptured), and line-like reticulated vessels disposed in one plane, so as to communicate laterally with the polyp-cells, and divide the axis longitudinally into equal halves. The more central canals of this vascular plane combined to form a compound vessel, which opened into a spherical sinus with cellular parietes at the base of each branch.

The ramification of the Polypidom generally exhibited a trichotomous arrangement, with simple articulations occurring only where the branches were given off.

The cells were clustered in linear series on opposite sides of the branched axis, oval in shape, corneous in texture, with a terminal combed aperture, folding inwards by the contraction of four equidistant sets of muscular fibres, which imparted a quadrilateral figure to the opening.

The polypes were very minute, but exhibited distinctly all the important points of structure observable in those of *Vesicularia* and *Bowerbankia*, between which genera this polyzoon would appear to

lie. The ciliated tentacula, like those of *Vesicularia*, are eight in number, and do not possess the motionless hair-like processes which project from the back of each in *Bowerbankia*.

Although too much importance must not be attached to the actual number of tentacula surrounding the oral aperture, the tendency to multiply those organs must not be altogether forgotten. Thus, while there are but eight in *Vesicularia*, *Bowerbankia densa* and *Bowerbankia repens* possess respectively ten and twelve.

Both *Bowerbankia* and *Vesicularia* agree in the uniserial and unilateral distribution of the polypes, but in the present instance the cells are arranged in linear and bilateral clusters.

February 26, 1857.

The LORD WROTTESLEY, President, in the Chair.

The following communications were read :—

I. "Observations on the Natural Affinities and Classification of Gasteropoda." By JOHN DENIS MACDONALD, Assistant Surgeon R.N. Communicated by Captain DENHAM, R.N., F.R.S. Received January 13, 1857.

(Abstract.)

During his sojourn among the Feejee Islands, the author devoted much time to the anatomical investigation of recent Gasteropoda, with the view of discovering such indications of affinity in the details of structure as might serve as a basis for a natural arrangement of the order; and the present paper is designed to give a statement of some of the results of his researches, in order that the affinities of structure developed may be fairly examined and taken for what they are worth as principles of classification.

After pointing out objections to the foundation of primary divisions among the Gasteropoda on characters derived from the shell or from modifications of the respiratory organs, the author observes in respect of the value of sexual characters, that when the distinguishing features of a class are once satisfactorily determined, and this contains

forms in which the sexes are either separate, or combined in the individuals, no other characters can be of greater importance in establishing primary divisions. As a means of further subdivision according to natural affinities, he suggests distinctive characters derivable from the auditory sacs and concretions, and from the oral, lingual and gastric dental organs.

In Mollusca, as in Fishes, the auditory concretions present themselves in one of two forms, viz. solitary lapilli, usually named *otolithes*, or groups of small granules of a rounded oval or irregular shape, which have been designated by the term *otoconia*. The lingual teeth are either set together on a short and broad lingual membrane, and form what the author calls a *lingual pavement*, or on a narrow longitudinal band termed *lingual ribbon* or *strap*. The latter usually consists of a median *rachis* flanked by two lateral portions or *pleuræ*; but in some cases the *rachis*, and in others the *pleuræ* are absent, and the number of longitudinal rows of teeth in these divisions may also differ in different genera. The fore part of the lingual membrane is supported by cartilage, so curved and fashioned as to receive the lingual sac behind and form a basis to the tongue itself projecting in front. This lingual cartilage may consist of a single piece thinned in the middle line, or of two or four distinct pieces, similarly arranged and wrapped together by muscle and ligament. The oral dental organs or *labial plates* are disposed either horizontally or laterally. In the former case a single plate may occupy the upper lip, or there may be two guarding the aperture of the mouth, and corresponding with both upper and lower lip, but the lateral plates are always in pairs. Gastric teeth occur in the Aplysiadæ and Bullidæ.

After pointing out further differences in the form and arrangement of the dental apparatus in different genera, the author thus describes the mode of development of the lingual teeth. "The lingual sac at first appears as a little cæcal process appended to the inferior part of the œsophagus, where it joins the oral cavity. In the median line of the floor of this sacculus, a few minute plates disposed in a longitudinal row form the rudiment of the future rachis, and the progress of their development may be distinctly traced on examining them, *seriatim*, from before backwards, in which direction, as their growth advances, they acquire a more perfect form. The internal row of pleural plates now makes its appearance, their development proceeding in a similar way; and after this follow the others according to their posi-

tion, the more internal arising first. Thus the whole ribbon of dental organs increases in length and breadth by additions made respectively to its anterior extremity and sides; and each transverse row gradually moving backwards by the continued development and growth of others anterior to it, causes elongation of the lingual sac, which only attains its perfect state when these processes are at an end. The idea, therefore, that the new teeth are developed from behind forwards and successively brought into use, as in sharks and rays among fishes, does not appear to me to be correct."

In the annexed Table a rough arrangement is given of a considerable number of genera grouped together by the characters above referred to. Although the author thinks it improbable that any genera opposed to each other in those fundamental particulars can be intimately related, yet the facts are not advanced as the basis of a new classification, but simply that they may yield their own weight, as so many available tests of affinity.

Sexes separate.				Sexes combined.			
Otolithes.		Otoconia.		Otoconia.		Otolithes.	
Strap.	Pavement.	Pavement.	Strap.	Strap.	Pavement.	Pavement.	Strap.
Oxygyrus.	Triphoris.	Solarium.	Helicina.	Patella.	Limax.	Ianthina.	Vermetus.
Atlanta.	—	—	Navicella.	Dentalium.	Parmacella.	—	Serpuloides.
Carinaria.	—	—	Neritina.	—	Helix.	—	Pileopsis.
Cardiapoda.	—	—	Nerita.	—	Helicella.	—	Hipponyx.
Firola.	—	—	Trochus.	—	Carocolla.	—	Pedicularia.
Cerophora.	—	—	Delphinula.	—	Bulimus.	—	Glaucus.
Egea.	—	—	Turbo.	—	Partula.	—	Eolidia.
Syncera.	—	—	Stomatella.	—	Vertigo.	—	&c.
Melania.	—	—	Broderipia.	—	Physæ.	—	—
Truncatella.	—	—	Haliotis.	—	Planorbis.	—	—
Planaxis.	—	—	&c.	—	Scarabus.	—	—
Litiopa.	—	—	Cyclophorus.	—	Conovulus.	—	—
Litorina.	—	—	Diplommatina.	—	Siphonaria.	—	—
Lacuna.	—	—	Pupina.	—	Amphibola.	—	—
Natica.	—	—	—	—	Tornatella.	—	—
Triton.	—	—	—	—	Dolabella.	—	—
Ranella.	—	—	—	—	Bulla.	—	—
Terebellum.	—	—	—	—	Cylichna.	—	—
Strombus.	—	—	—	—	Doris.	—	—
Mitra.	—	—	—	—	Pleurobranchus.	—	—
Harpa.	—	—	—	—	Phyllirrhoe.	—	—
Oliva.	—	—	—	—	—	—	—
Ricinula.	—	—	—	—	—	—	—
Purpura.	—	—	—	—	—	—	—
Murex.	—	—	—	—	—	—	—
Muricidea.	—	—	—	—	—	—	—
Cyrtulus.	—	—	—	—	—	—	—
Tritonidea.	—	—	—	—	—	—	—
Buccinum.	—	—	—	—	—	—	—
Nassa.	—	—	—	—	—	—	—
Pusiostoma.	—	—	—	—	—	—	—
Columbella.	—	—	—	—	—	—	—
Conidea.	—	—	—	—	—	—	—
Pleurotoma.	—	—	—	—	—	—	—
Terebra.	—	—	—	—	—	—	—
Conorbis.	—	—	—	—	—	—	—
Conus.	—	—	—	—	—	—	—

The following Table exhibits a further subdivision of the first series, according to the characters of their dental organs.

- Lingual dental organs.
  - Biserial.
    - Pleurotoma. Terebra. Conorbis. Conus.
  - Triserial.
    - Rachis, unarmed.
      - Columbella. Conidea.
    - Rachis, armed.
      - Strap, long and narrow.
        - Plural plates, articulated, moveable.
          - Buccinum. Nassa. Pusiostoma.
        - Broad plates in the Pleuræ, apparently fixed.
          - Muricidea. Cyrtulus. Tritonidea.
        - Simple hooks in the Pleuræ.
          - Harpa. Oliva. Ricinula. Purpura. Murex.
      - Strap, short and broad.
        - Mitra.
  - Septiserial.
    - Lateral, labial plates.
      - Egea. Syncera. Melania. Truncatella. Planaxis. Litiopa. Litorina. Lacuna. Natica. Triton. Ranella. Terebellum. Strombus.
    - Mouth, unarmed.
      - Oxygyrus. Atlanta. Carinaria. Cardiapoda. Firola. Cerophora.

Murex, Triton, and *Ranella* have always been associated together as members of one family by universal consent, and it must be confessed that the external resemblance between them is very remarkable; but on comparing the lingual and labial dental organs of *Triton* or *Ranella* with those of *Murex,* it will be at once perceived that the latter genus can have no immediate affinity with either of the former genera.

The aperture of the proboscis in *Murex* is transverse, and armed with two horizontally arranged dental plates, connected laterally by the minute semi-calcified cells which line this part. The upper plate presents a rough palatal surface, with an anterior encrusted cutting border, much resembling that of the crescentic mandible of *Limax* or *Helix*; whereas the dental plates of *Triton* and *Ranella* consist of oblique rhombic cells identical in character with those of *Cyclophorus* or *Natica,* disposed laterally. Again, on comparing the lingual strap of *Murex* with that of *Triton* or *Ranella,* we remark, first, that each transverse row of the former consists of three members, viz. one in the rachis, and one in each pleura, while in the two latter cases the pleuræ present two additional elements; thus there are seven series of dental plates in the strap. The tongue-strap of *Murex,* moreover, is lengthy like that of *Purpura* and *Ricinula,* both of which genera are more closely allied to *Murex* than perhaps any others referred to its family.

The strap of *Triton* and *Ranella,* on the other hand, is comparatively much shorter, and singularly enough more nearly approaches that of *Pileopsis* or *Vermetus,* not only in general proportions, but also in the actual number and configuration of the dental plates and processes. Now with these facts before us, it will be scarcely worth while entering further into the characters of the lingual dentition of *Murex, Triton,* and *Ranella,* but the most superficial examination of the figures (which accompany the communication) will show that *Murex* must be separated from its assumed alliance with *Triton* and *Ranella,* while the close relationship of the two latter genera gains additional support.

On comparing the lingual dentition of the genus *Cyrtulus* with that of *Tritonidea* of Swainson (the *Polia* of Gray), both are found to be naturally allied by characters which very distinctly manifest a family relationship, and Swainson's genus *Muricidea,* with several

others, must also be referred to this group. The lengthy triserial ribbon of *Cyrtulus*, or *Tritonidea*, for example, exhibits no true or immediate affinity with the comparatively short and septiserial dental armature of *Triton* or *Ranella*. Thus the author is induced to dissent from Mr. Gray's view that *Tritonidea* is allied to *Triton*, but agrees with him that the *Buccinidæ*, forming an equally characteristic natural family, are very close at hand.

The lingual dentition, and in fact the whole anatomy of *Terebra*, most unequivocally refer it to the *Conidæ*, and not to the *Buccinidæ*, amongst which it is at present received.

The author has not been able to detect lingual cartilages of the usual character in *Conus*, *Conorbis*, or *Terebra*, but the walls of the tongue-sac are stout, tough, and distinctly cartilaginous in structure; indeed the whole organ, including its armature, very much resembles the dental cheek-pouches of some Pteropods.

The lingual ribbon of *Pleurotoma* is exceedingly minute, and the parietes of the sac are not of that dense and unyielding character which they exhibit in *Conus*, *Conorbis**, and *Terebra*. Moreover, in *Pleurotoma* the little lingual membrane is supported by two rounded masses of cartilage composed of large spheroidal cells. The rachis appears to be absent altogether, and there is but a single row of elongated, slightly curved, and sharp-pointed teeth (differing considerably from those of *Conus* and *Terebra*) in the pleuræ.

The tongue-strap of *Mitra*, although remarkably short, is triserial like that of *Murex*, *Purpura*, &c.; but the author has invariably found that in those *Mitræ* in which the sculpturing of the shell was transverse, the pleural teeth were simple, uncinate, and mobile, while in those species characterized by a smooth surface or longitudinal sculpturing, the dental processes were small, straight, and numerous, arising just within the posterior border of broad basal plates. This difference is exactly such as exists between the lingual dentition of the respective groups to which *Murex* and *Tritonidea* belong.

Harpa and *Oliva* are very closely allied, by the general configuration of the body and the characters of the lingual dentition, though it must be remembered that the tongue-strap in the former is so very

* Several specimens of a recent species of this genus (hitherto known only in a fossil state) were obtained from depths ranging between 10 and 20 fathoms, within the barrier reefs surrounding the Feejee Islands.

minute, compared with the whole bulk of the animal, as to appear quite rudimentary. The simple lateral uncini, moreover, are only distinctly visible towards the posterior extremity of the sac. Both these genera seem to be more intimately related to *Murex* and its congeners than to the *Buccinidæ*.

Triphoris is now, as it would appear from the characters of its shell alone, placed with *Cerithium*, but the comparison of the internal anatomy of those genera offers no countenance to their supposed affinity; thus, single spherical otolithes occupy the auditory sacs of *Triphoris*, while those of *Cerithium* contain otoconia. The proboscis of the former is long and retractile. The lingual membrane of *Triphoris* besides, though long and ribbon-like, supports a multiserial pavement of minute teeth, while that of *Cerithium* is septiserial, resembling in many particulars the tongue-strap of *Pupina* and allied forms.

The *Columbellæ* deserve to be elevated to the rank of a family, distinguished from the *Buccinidæ* by the unarmed rachis, and curved versatile pleural teeth of the tongue-strap.

Although not fully satisfied of the propriety of separating the genus *Conidea* from *Columbella*, the author thinks there can be no doubt that *Pusiostoma*, formerly placed with the *Columbellæ*, forms a very distinct genus clearly referable to the *Buccinidæ*.

As great difference of opinion has always existed as to the distribution of the sexes amongst Gasteropods, so far the author is unable to vouch for the whole truth of the arrangement above given, but he thinks that if there is anything incongruous in it, the correction of errors in that particular would seem to be most likely to restore harmony and support the truth of the system.

In the course of his inquiries the author was impressed with the fact, that various genera of terrestrial gasteropods, which agree with each other as far as regards their respiratory organs and mode of respiration, differ essentially in their general organization, whereas they are in this respect severally related to fluviatile and marine genera, which are obviously constructed on the same anatomical type.

In this way a terrestrial genus, having few structural points of agreement to connect it laterally, as it were, with others of the same habit, forms a member of a beautifully connected natural series, traceable from it through fluviatile and littoral forms to others which are altogether marine.

As an example of these relations the following Table is given, and it might have been extended and rendered more complete, had the author not preferred to limit it to such cases as have come under his own examination.

	Sexes combined.	Sexes separate.		
	Otoconia.	Otoconia.		Otolithes.
	Pavement.	Lingual ribbon.		Ribbon.
	Teeth numerous, uniform in character.	Rachis and Pleuræ multiserial.	Rachis uniserial, Pleuræ triserial.	Rachis uniserial, Pleuræ triserial.
Terrestrial…	Helix.	Helicina.	Cyclophorus.	Egea.
	Bulimus.	——	Diplommatina.	——
	——	——	Pupina.	——
Fluviatile…	Physa.	Navicella.	?	Melania.
	Planorbis.	Neritina.	——	——
	——	——	——	——
Leading to purely marine genera.	Scarabus.	Nerita.	Cerithium.	Hydrobia.
	Conovulus.	Turbo.	——	Planaxis.
	Siphonaria.	Stomatella.	——	Litiopa.
	Amphibola.	Broderipia.	——	——

This Table shows the natural affinities of four principal divisions of terrestrial gasteropods, proceeding, as it were, in parallel lines, without any very obvious lateral connexions, through fluviatile and littoral forms, conducting to certain marine genera distinguished by this alliance from all others having no terrestrial representatives, and being therefore more restrictedly marine. It may be remarked that the importance of the characters placed at the head of the Table has been proved by the comparison of other anatomical particulars in those genera, and so far their efficiency in other cases is substantiated.

The author adds the following observations on the anatomy of the *Siphonaria* and *Amphibola*, as bearing on their position in the first series of the foregoing Table :—

"*Siphonaria* appears to enjoy the power of breathing in both air and water with equal facility, and on examination, we find the respiratory surface so constituted as to afford a ready explanation of the fact. Thus, in connexion with a narrow, combed, or rather transversely plaited gill, numerous vessels ramify extensively, and

anastomose freely upon the roof of the respiratory chamber. The mouth is armed with lateral labial plates, and the lingual dentition is not unlike that of *Amphibola*, to which genus it is further related by the absence of tentacula, and the general configuration of its head.

"*Amphibola* exhibits a close relationship to the Pulmonifera in many essential anatomical points, but it has a veritable combed gill, which, arising from a deep recess on the left side of the branchial chamber, and thence passing obliquely forwards towards the right side, terminates in a pointed extremity, in front of which there is a small glandular body, probably a renal organ. The margin of the mantle may be traced continuously round the neck and the base of the foot, being attached in its entire extent, with the exception of a small portion which arches over a narrow respiratory opening on the right side of the nape. The lingual sac is small, like a cæcal process appended to the antero-inferior part of the œsophagus. The dental organs present a pavement of narrow basal plates with very long and gently curved cusps. The teeth of the central series are much larger than the rest, and exhibit a remarkable conformation; thus a rounded process projects in the middle and several minute denticulations arm its base on either side. I have not succeeded in detecting either lingual cartilages or labial plates in my spirit-preserved specimens, and but for the support furnished by analogy, I would incline to the belief that they are absent in the present case.

"The remark made by Mr. Woodward in his very valuable little work the 'Manual of Mollusca,' that the anomalous genus *Amphibola* has an unusually broad tongue armed with teeth similar to those of the snail, is not quite correct. The misconception most probably originated in the inspection of a preparation belonging to Mr. Wilton of Gloucester, and from which Mr. Woodward's figure has been taken, as 'part of the tongue of *Amphibola avellana*;' but having myself dissected several specimens of this very species obtained at New Zealand, I am satisfied that Mr. Wilton's preparation has been by some accident improperly named.

"The general scheme of the generative system in *Amphibola* corresponds very closely with that of *Helix*, *Bulimus* and such Pulmonifera. The ovarium is imbedded in the liver near the summit of its spiral turns, and a small convoluted oviduct leads downwards and forwards along its inner or concave side. The testis lies con-

siderably in advance of the ovary; the intromittent organ forms a prominence in the floor of the respiratory chamber, and finally the generative orifices open on the right side."

II. "On the Sea Saw-dust of the Pacific." By JOHN DENIS MACDONALD, Esq., Assistant Surgeon R.N. Communicated by Captain DENHAM, R.N., F.R.S. Received January 13, 1857.

(Abstract.)

In this communication the author gives a description (illustrated by figures) of the remarkable little algal so frequently met with in the South Pacific, scattered over the surface of the water in broad streaks and patches of a pale yellowish-brown tint, and which is known under the name of "Sea Saw-dust."

After adverting to the occurrence of a similar phenomenon in other parts of the globe, and citing the account given of the *Trichodesmium erythræum* of the Red Sea by MM. Evernor Dupont and Montagne, together with a description extracted from the 'Colombo Herald' of May 14, 1844, of what was obviously an example of a vegetable scum of the same kind occurring on the sea off Ceylon, the author remarks, that in the instances met with by himself he did not recognize the fœtid odour so generally and pointedly spoken of in the accounts of others. He then states results of his own observation as follows:—

"It was rather difficult at first to determine whether our species is to be referred to the Oscillatoridæ or the Confervidæ. In the latter, a linear series of tubular cells compose the filaments, which are thus said to be jointed, but in the former, although the filaments are tubular, simple and continuous without actual joints, a pseudo-jointed appearance is presented by the apposition of the little masses of contained colouring matter. Notwithstanding, having submitted the 'sea saw-dust' of the Pacific to microscopic examination on several occasions, I was much inclined to believe that the filaments were actually jointed; and this view is supported by the cir-

cumstance that an empty tubule, or one in which the parietes may be traced continuously without being interrupted by joints or internal septa, has never fallen under our notice; besides which the filaments are exceedingly brittle, usually suffering cleavage in the transverse direction. It, however, undoubtedly belongs to the Oscillatoridæ.

"When the filaments are first removed from the water, they may be observed adhering side by side in little bundles or fasciculi; and besides the colouring matter, the little cells, or at least the intervals between the septa, contain globules of air, which sufficiently account for their buoyancy; and, moreover, in this respect, although their abiding place is the open ocean, their habit can scarcely be regarded as very different from that of those species which flourish in damp localities exposed to the atmosphere.

"The filaments are all very short compared with their diameter, with rounded extremities; and when immersed some little time in fluid so that the contained air-bubbles make their escape or are taken up, the pale colouring matter appears to fill the cells completely, and a central portion, a little darker than the rest, may be distinctly perceived in each compartment intersected by a very delicate transverse partition.

"We have found this species off the coast of Australia and in Moreton Bay, amongst the Polynesian Islands, and on two separate occasions off the Loyalty Group, in nearly the same geographical position."

March 5, 1857.

The LORD WROTTESLEY, President, in the Chair.

In accordance with the Statutes, the Secretary read the following list of Candidates for Election into the Society:—

Thomas Graham Balfour, M.D.
Robert Ball, LL.D.
Henry Foster Baxter, Esq.
Lionel Smith Beale, M.B.
Samuel Husbands Beckles, Esq.
Charles Tilstone Beke, Ph.D.
George Boole, Esq.
Edward M. Boxer, Capt. R.A.
Samuel Brown, Esq.
George Bowdler Buckton, Esq.
Thomas William Burr, Esq.
William Dingle Chowne, M.D.
William Coulson, Esq.
Thomas Russell Crampton, Esq.
Richard Cull, Esq.
Thomas Davidson, Esq.
Hugh Welch Diamond, M.D.
James Dixon, Esq.
S. W. Fullom, Esq.
William Bird Herapath, M.D.
Rowland Hill, Esq.
George Grote, Esq.
Rev. Thomas Kirkman.
Henry Letheby, M.B.
Waller Augustus Lewis, M.B.
George Macilwain, Esq.
David Macloughlin, M.D.
William Marcet, M.D.
John Marshall, Esq.
John Penn, Esq.
William Peters, Esq.
Lieut. Bedford Pim, R.N.
Andrew Smith, M.D.
Robert Angus Smith, Esq.
Warington Wilkinson Smyth, Esq.
Charles Piazzi Smyth, Esq.
Henry Clifton Sorby, Esq.
John Welsh, Esq.
Thomas Williams, M.D.
Joseph Whitworth, Esq.
Forbes Benignus Winslow, M.D.
Bennet Woodcroft, Esq.
James Young, Esq.

The following communication was read :—

"On what the Colonial Magnetic Observatories have accomplished." By Major-General SABINE, R.A., Treas. and V.P.R.S. Received February 26, 1857.

It has been suggested to me, that a brief review of what has been accomplished by the Colonial Magnetic Observatories, instituted on the joint recommendation of the Royal Society and British Association, would be acceptable; and that the officer who has been entrusted with the superintendence of these establishments is the person from whom such a review may most properly be expected. Fully assenting to both propositions, I have readily undertaken the task; and have availed myself of the occasion to add a few remarks and suggestions on the measures which appear to be required for the further prosecution of the objects for which the observatories were recommended.

The magnetic investigations designed to be carried into execution by the Colonial Observatories recommended by the Royal Society embraced a much wider scope than had been contemplated by any previous institutions, or than had been provided for by the arrangements or instrumental means of any then existing establishment, whether national or private. Not, as previously, limited to observations of a single element (the Declination),—or combining at the most one only of the components of the magnetic force,—the instructions of the Royal Society, and the instrumental means prepared under its direction, provided for the examination, in every branch of detail, of each of the three elements which, taken in combination, represent, not partially but completely, the whole of the magnetic affections experienced at the surface of the globe, classed under the several heads of absolute values, secular changes, and variations either periodical or occasional,—and proceeding from causes either internal or external. To meet the requirements of inductive reasoning, it was needful that the results to be obtained should comprehend all particulars under these several heads, attainable by an experimental inquiry of limited duration. That no uncertainty might exist as to the objects to which, in so novel an undertaking, attention

was to be directed, the Report of the Committee of Physics, approved and adopted by the President and Council of the Royal Society, stated in a very few sentences, remarkable alike for their comprehensiveness and conciseness, the desiderata of magnetical science. It may be convenient to reproduce these, when desiring to show the degree in which the Observatories have fulfilled their contemplated purposes :—"The observations will naturally refer themselves to two chief branches, into which the science of terrestrial magnetism in its present state may be divided. The first comprehends the actual distribution of the magnetic influence over the globe, at the present epoch, in its mean or average state, when the effects of temporary fluctuations are either neglected, or eliminated by extending the observations over a sufficient time to neutralise their effects. The other comprises the history of all that is not permanent in the phenomena, whether it appear in the form of momentary, daily, monthly, or annual change and restoration; or in progressive changes not compensated by counter-changes, but going on continually accumulating in one direction, so as in the course of many years to alter the mean amount of the quantities observed."—Report, pp. 1, 2.

With reference to the first of these two branches, viz. the actual distribution of the magnetic influence over the globe at the present epoch, the Report goes on to state :—"The three elements, viz. the horizontal direction, the dip, and the intensity of the magnetic force, require to be precisely ascertained, before the magnetic state of any given station on the globe can be said to be fully determined . . . . and as all these elements are at each point now ascertained to be in a constant state of fluctuation, and affected by transient and irregular changes, the investigation of the laws, extent, and mutual relations of these changes is now become essential to the successful prosecution of magnetic discovery."

With reference to the second branch, viz. the secular and periodical variations, it is observed that—"The *progressive* and *periodical* being mixed up with the *transitory* changes, it is impossible to separate them so as to obtain a correct knowledge and analysis of the former, without taking express account of and eliminating the latter;" and with reference to the secular changes in particular, it is remarked—"These cannot be concluded from comparatively short series of observations without giving to those observations extreme

nicety, so as to determine with perfect precision the mean state of the elements at the two extremes of the period embraced; which, as already observed, presupposes a knowledge of the *casual* deviations."

It is clear from these extracts that in the discussion of the observations the first point, in the order of time, ought necessarily to be an investigation into "the laws, extent, and mutual relations of the *transient* and," (as they were called at the time the Report was written,) "*irregular* changes," as a preliminary step to the elimination of their influence on the observations from which a correct knowledge and analysis of the progressive and periodical changes were to be obtained. It will be proper to show therefore, in the first place, what the Observatories have accomplished in regard to the so-called casual or transitory variations.

Casual Variations.—All that was known regarding these phenomena at the period when the Report of the Committee of Physics was written, was, that there occurred occasionally, and, as it was supposed, irregularly, disturbances in the horizontal direction of the needle, which were known to prevail, with an accord which it was impossible to ascribe to accident, *simultaneously* over considerable spaces of the earth's surface, and were believed to be in some unknown manner connected, either as cause or effect, with the appearances of the aurora borealis. The chief feature by which the presence of a disturbance of this class could be recognized at any instant of observation,—or by which its existence might be subsequently inferred independently of concert or comparison with other Observatories,—appeared to be, the deflection of the needle from its usual or normal position to an amount much exceeding what might reasonably be attributed to irregularities in the ordinary periodical fluctuations. The observations which had been made on the disturbances anterior to the institution of the Colonial Observatories had been chiefly confined to the declination. A few of the German Observatories had recently begun to note the disturbances of the horizontal force; but as yet no conclusions whatsoever had been obtained as to their laws: in the words of the Committee's Report, the disturbances "apparently observe no law." By the instructions cited above, the field of research was enlarged, being made to comprehend the disturbance-phenomena of the *three* elements; and the importance of their examination was urged, not

alone as a means of eliminating their influence on the periodic and progressive changes, but also on the independent ground, that "the theory of the transitory changes might prove itself one of the most interesting and important points to which the attention of magnetic inquirers can be turned, as they are no doubt intimately connected with the general causes of terrestrial magnetism, and will probably lead us to a much more perfect knowledge of those causes than we now possess."

The feature which has been referred to as furnishing the principal if not the only certain characteristic of a disturbance of this class, viz. the *magnitude* of the departure from the usual or normal state at the instant of observation, has, in the discussion of the observations, been made available for the investigation of their laws: it has afforded the means of recognizing and separating from the entire mass of hourly observations, taken during several years, a sufficient body of observations to furnish the necessary data for investigating at three points of the earth's surface—one in the temperate zone of the northern hemisphere, a second in the temperate zone of the southern hemisphere, and a third in the tropics—the laws or conditions regulating or determining the occurrence of the magnetic disturbances. The method by which this separation has been effected has been explained on several recent occasions, and will be found fully described in the Phil. Trans. for 1856, Art. XV. By processes of this description, the disturbances of principal magnitude in each of the three elements, the Declination, Inclination and Total Force, have been separated from the other observations, at the three observatories of Toronto, Hobarton and St. Helena, and submitted to an analysis of which the full particulars will be found in the preliminary portions of the volumes which record the observations. By the adoption of a uniform magnitude as constituting a disturbance throughout the whole period comprised by the analysis, the amount of disturbance in the several years, months, and hours is rendered intercomparable. The result of this investigation (which could not be otherwise than a very laborious operation, since the observations at a single one of these stations, Toronto, considerably exceeded 100,000 in number, each of which had to be passed through several distinct processes,) has made known to us that the phenomena of this class, which may in future with propriety

and advantage receive the appellation of "*occasional,*" are, in their mean or average effects, subject to periodical laws of a very systematic character; placing them, as a first step towards an acquaintance with their physical causes, in immediate connexion with the sun as their primary exciting cause. They have—1, a *diurnal* variation which follows the order of the solar hours, and manifests therefore its relation to the sun's position as affected by the earth's rotation on its axis; 2, an *annual* variation, connecting itself with the sun's position in regard to the ecliptic; and 3, a third variation, which seems to refer still more distinctly to the *direct* action of the sun, since, both in period and in epochs of maximum and minimum, it coincides with the remarkable solar period of about ten, or perhaps more nearly eleven, of our years, the existence of which period has been recently made known to us by the phenomena of the solar spots; but which, as far as we yet know, is wholly unconnected with any thermic or physical variation of any description (except magnetic) at the surface of the earth, and equally so with any other cosmical phenomena with which we are acquainted. The discovery of a connexion of this remarkable description, giving apparently to magnetism a much higher position in the scale of distinct natural forces than was previously assigned to it, may justly be claimed on the part of the Colonial Observatories, as the result of the system of observation enjoined (and so patiently and carefully maintained), and of the investigation for which it has supplied the data; since it was by means of the disturbance-variations so determined, that the coincidence between the phenomena of the solar spots and the magnitude and frequency of magnetic disturbances was first perceived and announced (Phil. Trans. 1852, Art. VIII.).

The extent and mutual relation of the disturbance-variations of the three elements, even at a single station, supply a variety of points of approximation and of difference, which are well suited to elucidate the physical causes of these remarkable phenomena; but valuable as such aids may be when obtained for a single station, their value is greatly augmented when we are enabled to compare and combine the analogous phenomena, as they present themselves at different points of the earth's surface. To give but a single example:—there are certain variations produced by the mean effects of the disturbances which attain their maximum at Toronto during the hours of the

night; the corresponding variations attain their maximum, at Hobarton, also during the hours of the night, but with a small systematic difference as to the precise hour, and with this distinguishing peculiarity, that the deflection at Hobarton is of the opposite pole of the needle (or of the same pole in the opposite direction) to the Toronto disturbance; whilst at a third station, St. Helena, which is a tropical one, the hours of principal disturbance are those not of the night, but of the day. A very superficial examination is sufficient to show that for the generalization of the facts,—a generalization which is indispensable for their correct apprehension and employment in the formation of a theory,—the stations at which the phenomena are to be known must be increased. Those which were chosen for a first experiment were well selected to prove the importance of the investigation, and thus to lead to its extension. It is only at the Colonial Observatories that the disturbance-variations have hitherto been made out; and taking experience as our guide, we have before us the evidence of the means by which the inquiry may be further successfully prosecuted*.

Periodical Variations.—The anticipation expressed in the Report of the Committee of Physics, that for the purpose of obtaining a cor-

* The Colonial Observatories under my superintendence were originally four in number, viz. Toronto, St. Helena, Cape of Good Hope, and Hobarton. In July 1846 the detachment of the Artillery at the Cape of Good Hope was withdrawn by orders from England, and the charge of the magnetical and meteorological observations transferred to Mr. Maclear, the Government Astronomer at that station. The magnetical observations made at the Cape, when the magnetic observatory was one of those under my superintendence, were published in 1851, with a discussion of certain of their results; and the disturbance-variation of the declination at the Cape has since been deduced by my assistant, Captain Younghusband, Phil. Trans. 1853, Art. VI. Since the transfer to Mr. Maclear, Mr. Pierce Morton, a gentleman of considerable mathematical attainments, who has been added as an assistant to Mr. Maclear in that branch of the Cape observations, has applied himself to the investigation of the lunar magnetic influence (as derived from the Cape observations), with a view of presenting the results to the Royal Society. For this, and other deductions,—such as, for example, the laws of the disturbances of the inclination and total force,—he will have the entire series of observations, viz. those as above-stated already published, and those which have been made since the transfer of the Observatory, up to the present time.

rect knowledge of the *regular periodical variations*, it would be found necessary to eliminate the "casual perturbations," has been fully confirmed. Had the latter been strictly "casual" (or accidental, in a sense contradistinguished from and opposed to periodical), a sufficiently extended continuance of observation might have occasioned their mutual compensation; but now that we have learned that the mean effects which they produce are governed by periodical laws, and that these laws and those of the regular periodical variations are dissimilar in their epochs, it is manifest that in their joint and undivided effects we have two variations, due to different causes and having distinct laws, superimposed upon each other; *to know the one correctly we must necessarily therefore eliminate the other.* A striking illustration of the importance of such elimination is furnished by the solar-diurnal variation of the total force. It will readily be imagined that the question must be an important one, whether a variation, which is supposed to derive its origin from the sun, be a single or a double progression; whether it have two maxima and two minima in the twenty-four hours, or but one maximum and one minimum in that period. When no separation is made of the disturbances, the progression appears to be a double one, having two minima, one occurring in the day and the other in the night. With the removal of the disturbed observations the night minimum disappears, and we learn that the regular solar-diurnal variation of the total force has but one notable inflection in the twenty-four hours, viz. that which takes place during the hours when the sun is above the horizon. The night minimum is in fact the mean effect of the occasional disturbances. It is probable that the nocturnal inflection of the solar-diurnal variation of the Declination may be ascribed to the same cause, namely to the superposition of two distinct variations.

A careful analysis of the solar-diurnal variations of the Declination at the Colonial Observatories has brought to light the existence at all these stations, of an *annual inequality* in the direction of the needle concurrent with changes in the sun's declination, having its maxima (in opposite directions) when the sun is in or near the opposite solstices, and disappearing at or near the epochs of the equinoxes. An intercomparison of the results of the analysis at these stations has shown, that this inequality has the remarkable characteristics of having notably the same direction and amount in

the southern as in the northern hemisphere, and in the tropical as in the temperate zones. An ingenious explanation of these phenomena has been suggested by Dr. Langberg of Christiania (Proceedings of the Royal Society, vol. vii. p. 434); but whether this explanation be or be not the correct one, the theoretical importance of the facts cannot be doubted, inasmuch as they appear to be wholly irreconcileable with the hypothesis which would attribute the magnetic variations to thermic causation. We may ascribe to the general and almost exclusive prevalence of the thermic hypothesis, and to its influence on magnetic reasonings, that the well-known erroneous opinion was so confidently promulgated by a deservedly high magnetic authority*, that a line *must* exist surrounding the globe, in which the needle would be found to have *no diurnal variation.* We have now, on the contrary, reason to be assured, by the facts of the annual inequality thus discovered, that there is no such line; but that everywhere in the regions of its supposed existence a diurnal variation subsists, having opposite characteristics in opposite parts of the year as influenced by the sun's position on either side of the equator, and disappearing only at the epochs when the sun passes from south to north or from north to south Declination.

Lunar Variation.—But if thermic relations have failed to supply a connecting link between the sun and those magnetic variations which are, without doubt, referable to the *sun* as their primary cause, the failure of that hypothesis is made still more obvious by the existence of variations governed by the *moon's* position relatively to the place of observation. We are indebted to M. Kreil, now holding the same position in Austria that I have filled in England, for the first suggestion of the existence of a lunar-diurnal variation of one of the elements, viz. of the Declination, founded on observations at Milan and Prague; and in the Phil. Trans. for 1856, Art. XXII., will be found an exposition of the facts of the moon's diurnal influence on each of the three magnetic elements at Toronto, viz. on the Declination, Inclination, and Total Force. In the case of this investigation, notwithstanding the smallness of the values concerned, the instrumental means supplied to the Colonial Observatories have been found competent to determine, with an approximation suffi-

* Arago, Annuaire, 1836, p. 284.

cient for present theoretical purposes, the character and amount *for each element* of the regular daily effect of the moon on the terrestrial magnetic phenomena, the existence of which does not appear to have been even suspected at the time when the Report of the Committee of Physics was drawn up. The *discovery* of the moon's influence on any of the magnetic elements is due, as already stated, to M. Kreil; but Toronto is the first, and as yet the only, station, at which the numerical values at every lunar hour of the lunar-diurnal variations of the three elements have been published. Corresponding statements to that which has been given for Toronto, will be found for St. Helena and Hobarton, in the volumes of those observatories, which are now in preparation. All the results at the three stations present the same *general* characters. The lunar influence does not appear to participate in the decennial inequality which is found in all the solar variations (Phil. Trans. 1857, Art. I.). The lunar-diurnal variation of each of the elements is a double progression in the twenty-four hours, having epochs of maximum and minimum symmetrically disposed. In *character*, therefore, it differs from what might be expected to take place if the moon were possessed of inherent magnetism, *i. e.* if she were a magnet, as it is usually termed, *per se*; and accords with the phenomena which might be expected to follow if she were magnetic only by induction from the earth. On the other hand, it is believed that the *amount* of the variation, as observed at each of these stations, very far exceeds what can be imagined to proceed from the earth's inductive action reflected from the moon. In this theoretical difficulty we are naturally thrown back to seek a more extensive knowledge of the phenomena than we have yet obtained, and to the generalization which will follow, when sufficient materials for it have been procured. In subordinate particulars, a difference, which is apparently systematic, is perceived to exist in regard to the hours which constitute the epochs of maxima and minima at the three stations, as well as in regard to the amounts of the respective variations; these differences are no doubt intimately connected with the causes of the phenomena, and are likely to lead to their elucidation. It is therefore greatly to be desired that the number of stations furnishing complete determinations, such as the Colonial Observatories only have hitherto supplied, should be increased.

The domain of periodical variations has thus been considerably enlarged since the Report of the Committee of Physics was drawn up; and must henceforth be understood to comprise, in addition to the variations "whose amount is a function of the hour-angle of the sun, and of his longitude" (or of his declination) (Report, p. 10),—1st, those variations of the three elements whose amount is a function of the hour-angle of the moon; 2ndly, those variations which were classed in the Committee's Report as "irregular," or "apparently observing no law," but which are now known to be governed by laws depending on the sun's declination, and hour-angle; and 3rdly, those variations, both "regular" and "occasional," which have their epochs and amounts dependent apparently on a solar period of not yet perfectly ascertained duration, manifesting itself also by periodical changes in the frequency and amount of the solar spots. With the exception of the last-named class, all these variations require, for their generalization, that the phenomena should be investigated at several points of the earth's surface widely distant from each other; and we have now the knowledge, grounded on experience, that a very few years are sufficient for the observations at each station, with the instrumental means and methods recommended by the Royal Society, and when the investigation is made a primary object by those who engage in it.

Absolute Values and Secular Changes.—But interesting and valuable as is the acquisition of a fuller and more precise knowledge of the comparatively small magnetic variations produced at the surface of the earth by the action or influence of external bodies, even greater importance seems to attach,—when *terrestrial* magnetism is in question,—to the purposes of that distinct branch of the duties of a magnetic observatory, which consists in the determination of the absolute values and secular changes of the three magnetic elements. By the *absolute values* we seek to acquire a knowledge of the actual present order and distribution of the terrestrial magnetic influence at the surface of the earth, and to provide the materials by which the constancy, or otherwise, of the earth's magnetic charge may hereafter be examined; and by determinations of the present direction and amount of the *secular changes,* we seek to become acquainted with the laws, and ultimately with the causes, of that

most mysterious change, by which the magnetic condition of the globe at one epoch passes progressively and systematically into that of another. It is specially by determinations of this class, obtained with the necessary precision in different parts of the globe, that, in the words of the Committee's Report, "the patient inductive inquirer must seek to ascend to the general laws of the earth's magnetism." At the time when the Report of the Committee of Physics was written, doubts were reasonably entertained, whether the limited time, during which the Colonial Observatories were likely to be maintained in action, would be sufficient for the determination of the secular changes; and it was therefore very properly argued, that "these changes cannot be concluded from comparatively short series of observations without giving to the observations *extreme nicety,* so as to determine with perfect precision the mean state of the elements at the two extremes of the period embraced." It is with much satisfaction, and with a well-deserved recognition of the pains which have been bestowed by the successive Directors of the Toronto Observatory, and their Assistants, on this branch of their duties, that I am able to refer to the determinations of the absolute values and secular changes of the three elements contained in the third volume of the Toronto Observations, in evidence that the instrumental means which were devised, and the methods which have been adopted, have proved, under all the disadvantages of a first essay, sufficient to determine these data with a precision which is greatly in advance of preceding experience, and, as far as may be judged, equal to the present requirements of theoretical investigation. This is the more deserving of notice, because Toronto is a station where the casual and periodical variations, which it was apprehended would seriously interfere with the determination of absolute values, are unusually large. We may derive, therefore, from the results thus obtained, the greatest encouragement to persevere in a line of research which is no longer one of doubtful experiment, and to give it that further extension which the interests of science require.

Amongst the results which have recompensed the labours of the Colonial Observatories in this branch of their inquiries, perhaps there is none of more importance in respect to the general theory of terrestrial magnetism, than the conclusion which has been established by means of the observations of the Declination at St. Helena, that

the current annual amount of secular change takes place by *equal aliquot portions in every month, and even in every fortnight of the year.* The magnitude of the annual change of the Declination at St. Helena, 8′ (or more precisely 7′·93 in each of the eight years in which the observations were maintained), and the comparative tranquillity of the tropical regions in regard to magnetic disturbances, were circumstances which rendered St. Helena a particularly eligible locality for an investigation of this nature. The result has been, to remove secular change altogether from the category of atmospheric or thermic relations, with which, in the absence of a correct knowledge of the facts, it has frequently been erroneously associated; and to show conclusively that it is a phenomenon of far more systematic order and regularity than has been generally apprehended (Proceedings of the Royal Society, vol. vii. pp. 67–75).

It has thus been shown, that, in each and all of the branches of inquiry for which the institution of the Colonial Observatories was recommended, they have accomplished the objects which were contemplated, and have in many respects exceeded the expectations on which their recommendation was founded. Nor has the scope of their performance been limited to a mere registry of the observations, or to their publication in a crude and undigested form. It was well remarked by an authority of the greatest weight, when addressing the British Association on the occasion of the assembly of the Magnetical and Meteorological Conference at Cambridge in 1845 (Herschel, Address, p. xxxv), that "A man may as well keep a register of his dreams, as of the weather or any other set of daily phenomena, if the spirit of grouping, combining, and eliciting results be absent." To advance by the simple and straightforward path of inductive inquiry, in a science such as terrestrial magnetism in which a physical theory has yet to be sought, the endeavour must be made "to grapple with the palpable phenomena, seeking means to reduce their features to measurement; the measurements to laws; the laws to higher generalizations; and so, step by step, to advance to causes and theories." The mere observational part is not, and ought never to be, viewed as the fulfilment of the duties of institutions such as magnetic observatories; those duties ought always to be held to include (either on the part of the Directors of the Observatories them-

selves, or on that of persons who, as Superintendents or otherwise, have constantly watched the progress of the work) "the systematic deduction from the registered observations, of the mean values, and of the local coefficients of diurnal, annual, and secular change;" because "no other class of persons stands in anything like so favourable a position for working out the first elementary laws of phenomena, and referring them to their immediate points of dependence," as those who have directed or superintended the processes by which the data required for the knowledge of the phenomena have been obtained. The introductory discussions prefixed to the several volumes which contain the observations at the Colonial Observatories, and a succession of papers presented to the Royal Society, and published in the Philosophical Transactions, bear testimony to at least unsparing labour, on the part of the Superintendent, to give a completeness to the experiment of Colonial Observatories, corresponding to its original conception; though no one is more sensible than himself that this portion of the duty might well have fallen into abler hands. One great advantage in the task has undoubtedly been enjoyed, viz. the union of the detailed knowledge above alluded to, with the opportunity of generalization and consequent insight, afforded by results admitting of strict comparison and combination, obtained from well-selected stations at such distant points of the globe, and by a uniform system of observation.

It may be useful on the present occasion to recall to more distinct recollection the views and opinions entertained by those who were the principal instigators of the proceedings by which the Royal Society became the responsible advisers,—and Her Majesty's Government the chief supporters,—of measures which have placed this country in the very conspicuous position of taking that lead in the advancement of certain branches of science, which other nations were willing and desirous that she should take. These views cannot be better stated than in the words of one to whom all will be willing to concede pre-eminence, as well in counselling the recommendation to Government as in conducting the several points connected with it to a successful issue (Herschel, in Quart. Review, No. CXXXI.). "Great physical theories, with their trains of practical consequences, are pre-eminently national objects, whether for glory or utility. In effect, such they ought to be considered by every nation calling itself civi-

lized; and if we look to consequences, we have only to point to the history of science in all its branches to show, that every great accession to theoretical knowledge has uniformly been followed by a *new practice,* and by the abandonment of ancient methods as comparatively *inefficient* and *uneconomical.* This consideration alone we think sufficient to justify, even on utilitarian grounds, a large and liberal devotion of the public means to setting on foot undertakings and maintaining establishments, in which the investigation of physical laws, and the determination of exact data, should be the avowed and primary object, and practical application the secondary, incidental, and collateral one. That the time is now fully arrived when other great branches of physical knowledge must be considered as entitled to share in that public support and encouragement which has hitherto fallen to the lot of astronomy alone, will, we think, be granted without hesitation by all who duly consider the present state and prospects of science. The great problems which offer themselves on all hands for solution—problems which the wants of the age force upon us as practically interesting, and with which its intellect feels itself competent to deal—are far more complex in their conditions, and depend on data which, to be of use, must be accumulated in far greater masses, collected over an infinitely wider field, and worked upon with a greater and more systematized power, than has sufficed for the necessities of astronomy. The collecting, arranging, and duly combining these data are operations, which, to be carried out to the extent of the requirements of modern science, lie utterly beyond the reach of all private industry, means or enterprize. Our demands are not merely for a slight and casual sprinkling to refresh and invigorate an ornamental or luxurious product, but for *a copious, steady, and well-directed stream, to call forth from a soil ready to yield it, an ample, healthful, and remunerating harvest.* There are secrets of nature we would fain see revealed,—resources hidden in her fertile bosom for the well-being of man upon earth, we would fain see opened up for the use of the generation to which we belong. But if we would be enlightened by the one, or benefited by the other, we must *lay on power,* both moral and physical, without grudging and without stint."

If at the period when it was still doubtful what the Colonial Observatories then just established might be able to accomplish,—and

when, in effect, the expectations from them were little more than the anticipations of what a voyage of discovery upon an unknown ocean might produce,—the propriety of embarking upon such investigations was thus unhesitatingly affirmed, how much more confidently may the duty of *perseverance* be insisted upon, when the results of the first experiment have already more than realized the hopes which caused it to be undertaken. They have indeed confirmed the belief that "the gigantic problem proposed to be resolved" is of a nature to yield in its full extent only to "continued and persevering inquiry;" but at the same time they may be said, in a certain sense, to have narrowed the field of inquiry, by showing more distinctly than was previously apprehended, both what is desired to be known, and how and where it is to be sought. If the history of magnetical science is to be something more than a fragment, the research must be persevered in.

In considering the means by which the researches thus opened out may be most advantageously prosecuted, it is natural that we should look, in the first instance, to the adoption, at other selected stations, of arrangements similar to those which were instituted at the stations which were chosen for a first, and as it has proved, successful experiment; and with this view I may be permitted to restate the opinions which I submitted to the Magnetical and Meteorological Conference at Cambridge in 1845, as all that has since taken place has served to confirm them.

"Before I close this communication, I wish to advert to the expediency of extending the system of observation now in operation at Toronto, St. Helena, and the Cape of Good Hope, to other of the British Colonies, where the same objects can be accomplished in an equally effective and economical manner.

"In cases where the institution of similar establishments is strongly urged by the Governor of a Colony,—where competent persons are present and disposed to superintend the observations, and where soldiers of the Artillery are stationed whose services may be available, and whose employment has been shown to be economical and effective in a high degree in the execution of a laborious and exact routine of observation,—there is wanting only a supply of instruments,—the temporary allotment of a building to contain them, —extra pay, such as the individuals at the above-named Observatories

receive,—and an authorized connexion with a head-quarter establishment whence they may derive instruction and guidance.

"The cost of one of the Ordnance Observatories (including £100 a year for incidentals of all kinds) is £392 a year, exclusive of publication. It may be assumed that five years of hourly observation is a sufficient time of continuance for obtaining in any particular colony the mean values of the magnetical and meteorological elements, and their diurnal, annual, and secular variations, as well as the peculiarities of climate bearing on the health and industrial occupations of man. If the observations were printed *in full detail* for the five years, they would occupy two quarto volumes; but if it were thought sufficient hereafter that duplicate or triplicate manuscript copies should be deposited in different public libraries, and that publication should be confined to abstracts and an analysis, the cost of the publication would form but a small addition.

"The colonies of Ceylon, New Brunswick, Bermuda, and Newfoundland are in the described case; their respective Governors are recommending the establishment of Magnetical and Meteorological Observatories in them; competent directors are on the spot [this was written in 1845]; and they are all Artillery stations."

To the four stations thus named may be now added Mauritius and Demerara, as from both those Colonies, strong and repeated applications to the same effect have been sent through their respective Governors to the Secretary of State for the Colonies. Both these Colonies have offered to bear a portion of the expense of the proposed establishments; and have earnestly solicited to be placed in connexion with a head-quarter establishment, from which they might receive properly constructed instruments, with instructions and guidance in their use. Can it be said that we perform our duty as a mother-country when we put such applications on the shelf?—whilst, in the interests of science, it would be difficult to estimate too highly the value of such institutions,—in forming good observers, who might subsequently extend their activity over a wider range,—in affording to travelling observers the opportunity of testing and correcting their instruments, as well as keeping up and perfecting their skill in observation,—and in contributing to arouse, to nourish, and to extend to other parts of natural knowledge, that desire for the greatest pos-

sible accuracy, which was formerly met with only in astronomy and in geodesical operations of the highest class.

When it was first suggested that the officers and soldiers of the scientific corps of the army (Artillery or Engineers) stationed in the Colonies might, both beneficially to themselves and advantageously to the public interests, be made available for the performance of such temporary services, the suggestion, from its novelty, might have been open to many objections. None were, indeed, made by the military authorities of the time, who on the contrary approved and encouraged the proposition. There may have been doubts entertained in other quarters whether persons, whose ordinary occupations were so dissimilar, would be found to possess the necessary qualifications for carrying out a scheme of exact and varied observation, in which there was then no precedent to guide, and of which the performance would be sure to be extensively and closely scrutinized: but such doubts, if they existed, have probably long since subsided, as the successive volumes of the Colonial Observatories have appeared.

One great and unquestionable advantage which future institutions of this nature will have over those whose duties are accomplished, will be found in the assistance they will derive from the *Physical Observatory of the British Association* at Kew, as a head-quarter Observatory, in which their instruments can be prepared and verified, the constants, &c. carefully determined, new instruments be devised as occasion may require, and tested by experiment before they are sent out for use, and to which practical difficulties of all kinds, which may present themselves to the directors, may be referred.

The omission of a provision of this kind when the Observatories were first formed, was undoubtedly a great fault, which has been, and could only be, very imperfectly remedied by the Woolwich establishment, designed for a very different purpose, and insufficient even for the duties for which it was designed.

There is another advantage (if it be one) which might attend the early prosecution, viz. the opportunity of consulting (if it were desired to consult) the experience of the person who has conducted, —and, as he believes, successfully conducted,—the first experiment,

from its commencement now almost to its close; but this, in the course of nature, can only be available for a few years to come.

The Colonial establishments were instituted at the instance of the Royal Society and British Association, with a more general concurrence and approval on the part of the cultivators of science in all parts of the globe than, it is believed, were ever before manifested in regard to any purely scientific undertaking; and with such a cordial and effective cooperation of the public authorities as is well deserving of being held in remembrance. It is for those two great scientific bodies to consider whether any, and what, steps should now be taken to procure the continuance of the researches.

March 12, 1857.

Major-General SABINE, Treas. and V.P., in the Chair.

The following communications were read:—

I. "On the Immediate Principles of Human Excrements in the Healthy State." By W. MARCET, M.D., F.C.S., Assistant Physician to the Westminster Hospital. Communicated by H. BENCE JONES, M.D., F.R.S. Received February 23, 1857.

(Abstract.)

In a previous paper I had the honour of communicating to the Royal Society the results of a first series of investigations on the immediate principles of the fæces of man and animals; since then I have continued my researches on human excrements, being most ably seconded by my assistant, Mr. Frederick Dupré, Ph.D.

The new results obtained were the following:—

1. *Margarate of lime, phosphate of lime,* and *margarate of magnesia* were discovered to be immediate principles of human evacuations.

2. I found a new method for obtaining excretine, and its chemical formula has been established.

3. The fact that vegetable food induces the presence of margaric acid in excrements has been confirmed.

4. The existence of a comparatively large quantity of cholesterine in the spleen, which I had mentioned before as probable, has been confirmed.

When human fæces are exhausted with boiling alcohol, the fluid being rapidly strained through a cloth, a clear extract is obtained, which, on cooling, yields a deposit; this substance, being collected on a filter, is partly soluble in boiling alcohol, and there remains undissolved a residue insoluble in ether and alcohol. The residue in question being boiled with a solution of potash, dissolves almost entirely, and the addition of hydrochloric acid induces the formation of a precipitate in the solution. On examining this precipitate, it was found to consist of a crystallizable substance fusing at 60° Cent.; its structure and other properties were precisely those of *margaric acid*.

The acid filtrate contained *phosphoric acid* and *lime*. From several quantitative analyses, I concluded that there was more lime than is required to combine with the phosphoric acid in the form of the neutral phosphate, the excess of lime being exactly that which was necessary to convert the margaric acid into a neutral margarate of lime, $C_{34} H_{33} O_3 + CaO$. Consequently it followed that the three substances existed in the form of *margarate of lime* and *phosphate of lime* as immediate principles of human fæces.

The alcoholic filtrate from the deposit being allowed to stand for twenty-four hours, deposited another substance, of a nearly white appearance, and which proved to be *margarate of magnesia*.

The peculiar action of a vegetable diet on human fæces was investigated by means of experiments undertaken upon myself, when I observed that an entirely vegetable diet was attended with the formation of a large quantity of margaric acid in the excrements,—most probably not in the form of a margarate, but in the free state, inasmuch as it was obtained from the decomposition, with hydrochloric acid, of the precipitate induced by adding milk of lime to the cold and clear alcoholic extract of fæces, after the separation of the above-described deposits.

In the month of December 1855, I had an opportunity of noticing that during a cold night, when the temperature falls below the

freezing-point, *excretine* crystallizes readily and in large quantity in the clear alcoholic extract of fæces; this method I employed as often as possible, to prepare enough excretine for its chemical analysis; but the cold weather not lasting long enough, and this season having been remarkably mild, I was compelled to adopt a modification of the process by milk of lime, described in my former communication.

Having prepared a sufficient quantity of excretine, partly by the action of cold, and partly by means of milk of lime, the chemical composition of this substance was now determined. A qualitative analysis showed it to consist of carbon, hydrogen, sulphur and oxygen*; there was no water of crystallization present. Oxide of copper was employed at first for the combustions, but they were subsequently undertaken with chromate of lead, on account of the large proportion of carbon that excretine contains; no substance having been found to combine with it, its atomic composition was calculated from the assumption that one equivalent contained one equivalent of sulphur; and the following formula was obtained:—

78 eq. Carbon	468
78 eq. Hydrogen	78
1 eq. Sulphur	16
2 eq. Oxygen	16
Atomic weight of Excretine	578

I shall not add more at present as to the properties of excretine; the plate which accompanies the paper illustrates the shape and arrangement of its crystals.

In my former communication I had stated that when the tissue of the spleen is submitted to a process of analysis similar to that adopted for the extraction of excretine, a substance closely allied to cholesterine is obtained. This subject being one of great importance in a physiological point of view, I have resumed the investigation, and placed beyond doubt that this substance is really *cholesterine*. Its

* In my former communication I had erroneously stated that excretine contained nitrogen, which resulted from my not having been able to prepare a sufficiently large quantity of the substance; and, moreover, it might not have been perfectly pure.

presence in the spleen is evidently independent of that which might exist in the blood retained by this organ after death. Is it that the spleen secretes cholesterine? This can only be determined by actual experiment; but it is very remarkable that a part of the blood which is supplied to the liver should come directly from an organ containing large quantities of a substance known to enter into the composition of the bile.

II. "Description of a Chronometer Compass." By Ralph Reeder, Esq., of Cincinnati, U. S. Communicated by Capt. Washington, R.N., F.R.S. Received Feb. 26, 1857.

This instrument is a combination of the Universal Dial and Chronometer, and is intended to show the errors of the magnetic needle, both at sea and on land, and, in clear weather, to perform in place of the needle.

III. Extract of a Letter addressed to General Sabine, R.A. Treas. and V.P.R.S., by M. R. Wolf, dated Zurich, March 7, 1857. Communicated by Gen. Sabine.

J'avais l'honneur de vous envoyer le 2^{me} numéro de mes 'Mittheilungen über die Sonnenflecken,' dans lequel j'ai développé que mes observations des tâches du soleil dans les années 1849 à 1855 prouvent assez clairement, qu'il y a dans ces phénomènes curieux une période correspondante avec l'année terrestre, dont les deux minima correspondent aux deux époques où la terre passe par le plan contenant l'axe du soleil et une parallèle à l'axe de la terre,—les deux maxima aux deux époques où la terre s'éloigne le plus de ce plan. Depuis ce temps-là j'ai trouvé qu'il y a une période correspondante dans les variations du magnétisme terrestre. En combinant les variations en déclinaison observées sur l'hémisphère boréale avec celles de l'hémisphère australe, pour éliminer l'influence de la déclinaison du soleil, j'ai trouvé une période annuelle pour ces variations, dans laquelle les deux minima et le deux maxima se présentent encore plus claires que dans les tâches solaires, et de même encore plus rapprochés des

époques ci-dessus mentionées. Je crois que cette nouvelle correspondance entre les tâches solaires et le magnétisme terrestre, que j'avais déjà soupçonnée en 1853 (voir les 'Mittheil. der Nat. Ges. in Bern), suffira pour convaincre les derniers sceptiques qu'il y a une correspondance réelle entre ces deux phénomènes.

A peine avais-je terminé cette recherche et donné à l'Académie de Paris le même résumé que vous avez lû dans les dernières lignes et que j'aimerais voir communiqué par vous à la Société Royale, que je trouvai dans votre 3me mémoire sur les 'Magnetic Disturbances' la même période découverte par vous dans ces 'Disturbances.' Je suis bien heureux de vous rencontrer de nouveaux dans mes études, et je donnerai dans le 3me numéro de mes 'Mittheilungen,' qui paraîtra sous peu, et un extrait de votre lettre du 16 Décembre et la date de votre 3me mémoire.

IV. "Anatomical Description of a Species of Asteroid Polypes, probably forming the type of a new genus of Alcyonidæ." By John Denis Macdonald, Assist. Surg. R.N. Communicated by Capt. Denham, F.R.S. Received January 13, 1857.

On leaving the Conway Reef (lat. 21° 44′ 48″ S., long. 174° 37′ 45″ E.), July 4, 1855, a very beautiful branched asteroid Zoophyte, belonging to the Alcyonidæ, was brought up from a depth of between 30 and 40 fathoms, on the buoy-rope of the anchor.

The polypidom, from a trunk of about one inch and a half in diameter, branched off, with much irregularity, but generally in a dichotomous manner, into very minute subdivisions.

The investing membrane was strengthened by the close deposition of elongated, fusiform, and minutely tuberculated spicula of a deep crimson-lake tint, which impart their colour to the whole mass.

The internal substance was chiefly composed of longitudinal muscular septa, radiating from the central axis (which contained no denser material), frequently communicating with one another laterally, and being fixed into the internal surface of the integument in vertical lines. These muscular septa were invested on each side with a layer of finely reticulated vessels; both sets being connected by

numerous transverse trunks passing through the intervening muscular tissue, and the spaces between the septa were filled with a transparent glairy fluid.

The polyp-cells were exposed and solitary, resting on the internal surface of short branchlets strengthened by large dorsal spicula, one of which, much larger than the rest, extended considerably beyond the polyp-cells, tapering gently to a needle-like point. These latter spicula are covered with tubercles, and in every respect, but in size, similar to those of the general integument.

The small spicula on the internal or ventral surface of the branchlets diverge from one another in the peripheral direction, while those on the dorsal border are disposed longitudinally.

The mouth of each cup-like polyp-cell was surrounded with about eight projecting spicula, whose fixed extremities were curved upwards and inwards, festoon-fashion, while numerous smaller ones were so disposed as to fill up the open spaces posteriorly, and thus strengthen the body of the cell.

Although I have not been able to count the number of the oral tentacula satisfactorily, from their proportional size I can readily believe that there were about eight in this species, as in most if not all other asteroid polypes. They were broad and flat, tapering to a blunt point, like those of *Sarcodyction* (Forbes), to which genus I have no doubt this Zoophyte is nearly allied, though the habit of the polypidom is so very different.

March 19, 1857.

Dr. W. A. MILLER, Vice-President, in the Chair.

The following communications were read:—

I. "A System of Train-Signalling, by which also disabled Trains may telegraph for assistance without the aid of portable apparatus." By CHARLES V. WALKER, Esq., F.R.S. Received March 9, 1857.

(Abstract.)

When, in the early days of telegraphy, messages were sent and

trains were signalled on the same wires, no facilities existed for reducing the apparatus employed for the latter purpose, to a simple form. The case is now becoming different, special wires being largely devoted to train signals; hence the present system.

The *instrument* employed is a large electro-magnet, with a moveable armature, carrying a stem and a hammer, which latter strikes on a bell by the direct force of magnetism. It is provided with a contact-maker, a spring, the depression of which causes a current to circulate. The bobbins are 4 in. $\times$ 3 in.; and are filled with ten pounds of covered copper wire, No. 16 or No. 18. The core is of five-eight inch iron. The armature and appendages weigh $2\frac{1}{4}$ oz. Bells of this kind have been in action for five years without cleaning or repairing. The battery is zinc-graphite, and a solution of 1 sulph. ac. + 8 or 10 water. The plates, $7\frac{1}{2}$ in. $\times$ 3 in., are placed in stone pots that contain about a quart, the zinc standing in a gutta-percha slipper, containing mercury. Batteries of this kind will do their work untended for half a year and longer.

The *language* consists of blows on the bell; the number of blows varies according to the train-signal to be given. The distinctions required for ordinary purposes being few, the bell-language is very appropriate, from its addressing the ear, from its simplicity and from the facility with which the signals are given and taken. *One* blow is for the starting of an ordinary train; *two,* for an express; *three,* for the arrival of a train; *five,* for stopping all trains; *six,* for testing. This is a general code; other forms of code are used for protecting level crossings and junctions; but the fundamental signals of the general code are of universal application. This system was introduced five years ago on the South Eastern Railway; and at the present time consists of about 100 bells, to which additions are in progress.

The bells are connected in pairs, both bells being in a circuit that terminates in the earth in the usual way, at each station. The signal is made by depressing the spring from its earth-contact, upon the zinc end of the battery, the graphite end being in permanent connexion with the earth. The battery being thus introduced between the bell and the earth, a current circulates along the wire and produces one blow upon the bell. The home bell may be excluded or not from the circuit, when a signal is sent.

By the above arrangement signals are sent from station to station. But the extreme simplicity of the battery, the bell, and the language allows the arrangements to be so modified that signals may be made on a pair of bells from any joint, intermediate between two bell-stations, without the necessity of providing the signaller with any telegraph or battery, or any electrical apparatus whatever. The addition of this property to the bells does not in any way interfere with their being in perfect action and constant use for the ordinary work of train-signalling, and therefore if the guards of trains and the plate-layers of the permanent way are provided with a signal for expressing their wants, a great advance is made in telegraphy, and a large element of safety is gained for the travelling public.

It is well known to electricians that, if two equal and opposed currents are presented to the respective ends of a wire, no evidence is manifested of the circulation of electric force; the wire is in a null state, as much so as if no current was presented to it. Taking advantage of this law, in connexion with the simple bell-system above described, the circuit is made to contain the two batteries, one at each station, as well as the pair of bells; the same pole, the graphite, for instance, of each battery being connected with the earth.

When the home-station signaller desires to make a signal, he depresses the spring as before; but the connexions are such, that by this act he excludes his *own* battery from the circuit. The circuit then contains but one battery,—namely that at the pass station; the current of which is now able to circulate from end to end, being no longer counterbalanced by an equal and opposite current; and consequently the bells are sounded. This, then, is the process for ordinary train-signalling, under this arrangement.

By altering the contact-maker so that it inverts the battery in the circuit, instead of putting it out of circuit, both batteries are made available for each signal; and consequently the power and with it the cost of each may be reduced.

But the null state of the wire is equally well and very readily destroyed, by connecting it with the earth at any point intermediate between the two stations; for by this process a complete circuit is made or channel opened for the discharge of both ends of both batteries, each independently of the other, except that the attached wire between the earth and the telegraph wire is common to both

circuits, and thus the bells at the respective stations are actuated by the batteries of the respective stations. If ten blows with a pause of a minute, and then ten more, is the signal that the engine is disabled; ten blows, and a minute of contact, that an accident has happened; a ringing continued beyond ten, that the permanent way is obstructed, the stations at either side are advised and can take the measures necessary to meet the case.

These contacts may be made by hooking a wire or rod on to the line wire and making the necessary contacts with the rail; or, which is better, by establishing contact-makers, properly secured at frequent intervals on the telegraph posts.

This system gives to those in charge of disabled trains a certain means of asking for assistance from any point of the open railway, without any training beyond that of *counting ten* slowly and correctly. In practice, as between Red-Hill and Reigate, no inconvenience or loss of electricity has been suffered from counterbalancing the two currents.

The author states that there are other properties of opposed currents to be communicated on another occasion.

II. "On the Action of Aqueous Vapour in disturbing the Atmosphere." By Thomas Hopkins, Esq. Communicated by W. Fairbairn, Esq., F.R.S. Received January 2, 1857.

(Abstract.)

In this paper it was maintained that the great disturber of the equilibrium of atmospheric pressure is the aqueous vapour which is diffused through the gases. These gases, when ascending, cool (say 5°) through expansion by diminution of incumbent pressure, whilst the vapour that is within them cools only 1°; and a consequence is, that when a mixed mass ascends, the vapour is condensed by the cold of the gases. It is well known that condensation of vapour gives out much heat, and this heat warms and expands the gases when they are forced to ascend, taking vapour with them; and the process being repeated and continued, an ascending current is produced in the atmosphere, cloud is formed, the barometer sinks, rain falls, and winds blow towards the part.

This was shown to take place in all latitudes, producing disturb-

ances great in proportion to the amount of vapour condensed. In tropical regions, where the aqueous material is abundant, the disturbances are great, but take place principally in the higher regions of the air. The diminution of atmospheric pressure within the tropics at the surface of the earth, as measured by the barometer, extends over a large surface, but is not great in any one place. In cooler latitudes condensation takes place nearer to the surface of the globe, and then reduction of pressure is confined to a smaller area; but in parts on the surface within that area the reduction is great, because the lower and therefore heavier gases have been warmed and expanded; hence the falls of the barometer in certain cool localities are the greatest. In very cold and dry regions, as a consequence of there being but little vapour in the air to be condensed, the barometer sinks only a little, and that sinking is generally confined to a small area. In accordance with this view, it was shown that, in certain places, where much continuous rain falls, the barometer has a low average; and towards these areas winds blow from distant parts, as in the great trade- and other winds. Sea-breezes were also shown to be consequences of the condensation of vapour, which had been produced by the morning sun ascending to sufficient elevations; whilst the land-winds at night are attributable to the cooling of those elevated parts by evaporation during the absence of the sun.

Various objections that had been made to this theory of atmospheric disturbances were noticed by the author of the paper, which, though admitted to be plausible, were stated to be invalid, whilst the most important meteorological phenomena were asserted to be in accordance with it.

III. "On the Structure and Development of the *Cysticercus Cellulosæ* as found in the Muscles of the Pig." By George Rainey, Esq., M.R.C.S.E. Communicated by R. D. Thomson, M.D., F.R.S. Received January 16, 1857.

The observations detailed in this Communication were made known to the Society on a former occasion ('Proceedings,' Dec. 13, 1855), and are now reproduced with illustrative figures and suitable reference to contemporary researches on the origin and metamorphosis of the Cystic Entozoa.

IV. "On the Serpentines of Canada and their associated Rocks." By T. STERRY HUNT, Esq., of the Geological Survey of Canada. Communicated by THOMAS GRAHAM, Esq., F.R.S., Master of the Mint. Received February 26, 1857.

The origin and formation of serpentine is still regarded as an unsettled problem by chemical geologists, and Sir William Logan having shown from structural evidence the undoubted stratified character and sedimentary origin of the serpentine of the Green Mountains in Canada, I have been induced to make a chemical and mineralogical investigation of these serpentines and the rocks associated with them. In the present note I propose to indicate briefly some of the results obtained, reserving for another occasion the details of my examination.

The serpentines of the Green Mountains, which have been traced for 150 miles in Canada, have been found by Sir William to belong to the upper portion of the Lower Silurian system, whose disturbance and metamorphism have given rise to the great Apallachian chain, of which the Green Mountains are the north-eastern prolongation. These mountains are composed of gneissoid, micaceous, argillaceous, talcose, and chloritic schists, with quartzite, limestone, dolomite, serpentine, pyroxenite, and the other rocks about to be mentioned.

The serpentines, which form immense beds, and often cover large areas, are sometimes homogeneous, and at other times conglomerate in their character, the cement being a ferruginous dolomite, or more rarely a carbonate of magnesia, exempt from lime but containing carbonate of iron. In some cases the serpentine is intimately mixed with a large amount of carbonate of lime. Chromic and magnetic iron, ilmenite, diallage, with the ordinary lamellar and fibrous varieties, picrolite and chrysotile, are common in these serpentine rocks. The results of a great number of analyses show a uniformity of composition in all the serpentines of this formation, and also show a curious fact hitherto overlooked,—that of the constant presence of a small portion of nickel, never exceeding a few thousandths. I have never failed to detect it in any variety of serpentine from this formation, not only in Canada, but in the States of Vermont, Connecticut, and New Jersey. Its presence seems still more widely spread, for I have also found nickel in serpentines from California, the Vosges in

France, and in a *verd antique* marble from a Roman ruin. The association of nickel with the chromic iron and serpentines of Pennsylvania has been long known, and I have found the chromic iron of Canada to contain small portions both of nickel and cobalt, although the latter metal can rarely be detected in the nickeliferous serpentines.

The results of a number of analyses show that it is constantly present in the talcose slates and steatites of this region, and the same is to be observed of the magnesites and dolomites of the series; indeed the distribution of nickel would seem to be co-extensive with that of the magnesia in this formation, and the same thing may be said of chrome. I have not, however, met with any traces of chrome or nickel in the serpentines and talcose rocks of the Laurentian system, which underlies the Silurian and the still older copper-bearing rocks of Lake Huron, and probably corresponds to the oldest gneiss of Scandinavia. Both chrome and nickel, however, characterize the serpentines of the Vosges and of California. I have not yet been able to examine specimens from other foreign localities. The presence of traces of nickel in certain talcs was long since noticed by Stromeyer. Much of the so-called talcose slate of the Green Mountains is not magnesian, but consists of a hydrous aluminous silicate allied to pyrophyllite or pholerite, which are alumina-talcs.

The euphotides, which are associated with many of the European serpentines, are not wanting in the Green Mountains, although less distinctly marked to the eye than the foreign varieties. A tough, greenish or greyish-white rock, with a waxy lustre, forms in many places great stratified masses, which are associated with the serpentine, and is found on analysis to consist of a soda-felspar (albite) with a silicate of lime, magnesia, and protoxide of iron, having the composition of amphibole,—thus constituting a veritable euphotide. The two minerals are clearly distinguishable after calcination, which blanches the felspar, and reddens the ferruginous silicate. These rocks are by this means distinguished from others similar in their appearance and mode of occurrence, but consisting of petrosilex or compact siliceous felspar, and equally members of the sedimentary series. The specific gravity of these euphotides shows that the saussurite or felspar which forms their base has a density not greater than that of ordinary soda-felspar.

Immediately connected with the serpentines, there sometimes occurs a white compact rock, remarkable by its great hardness, and a density of 3·3 to 3·5. Analysis shows this rock to be a pure lime-alumina garnet, in some cases, however, mingled with another silicate which appears to belong to the amphiboles. This garnet is sometimes blended with serpentine, and at others forms distinct beds. In its general aspect it resembles closely the saussurite of the associated euphotides, and has probably often been confounded with that mineral by previous observers. Hence the densities of 3·2 and 3·3 assigned by different mineralogists to the saussurites of the Alps, while Delesse has shown that the true saussurite of the euphotide of Mount Genèvre, like that of the Vosges, is a felspar.

The magnesites of this region form great beds; they are crystalline, and consist of carbonate of magnesia with some carbonate of iron, and contain as imbedded minerals in some cases grains of quartz, in others felspar and talc, and at other times serpentine, but always holding chrome and nickel, the latter as a greenish carbonate, in the joints of the rock, or in the form of nickeliferous pyrites.

These magnesian rocks are not confined to the altered portions of this formation; beds of siliceous dolomite holding protocarbonate of iron are found, interstratified with pure fossiliferous limestones, near Quebec. The reaction between silica and the carbonates of lime, magnesia, and iron, which takes place at no very elevated temperature, in the presence of water, producing silicates of these bases with evolution of carbonic acid, enables us to understand the process which has given rise to the pyroxenes, serpentines, and talcs of this formation, while the argillaceous limestones, which are not wanting, contain all the elements of the garnet-rock.

The general conclusion deduced from these inquiries, and sustained by a great number of analyses, which I hope soon to submit to the Society, is, that the metamorphism of these Silurian strata has resulted from the chemical reaction, in the presence of water, of the elements existing in the original sedimentary deposits.

V. "On Determinants, better called Eliminants." By Professor FRANCIS NEWMAN, M.A. Communicated by Dr. BOOTH, F.R.S. &c. Received March 6, 1857.

(Abstract.)

1. This paper aimed at recommending the introduction into elementary treatises of the doctrine of Determinants; which, following Professor Boole, it called *Eliminants*. It exemplified the great aid to the *memory* which the notation affords. It undertook to show, that if only so much of new notation be used, as is needed in elementary applications, the subject becomes full as easy as the second part of algebra. The method of proceeding recommended may be understood by the following concise statement.

If n linear eqq. are given, connecting n unknown quantities; and every eq. is represented by $A_r x+B_r x_2+C_r x_3+\cdots+N_r x_n=P_r$ (where r is 1, 2, 3 ... n in the several eqq.), then, solving for any one of the unknowns, we of course obtain a result of the form $mx=a$. Very simple considerations then show, that m and a will be *integer functions of the coefficients*: namely, it is easy to prove, that *if* this is true for one number n, it must needs be true also for the number $(n+1)$; and consequently is generally true. Next, the same analysis exhibits, that $m=0$, is the result obtained, when $P\ P_2\ P_3 \ldots P_n$ all vanish: moreover, that if the system presented for solution be the $(n-1)$ eqq.

$$\left.\begin{array}{c} Bv+B_2v_2+B_3v_3+\ldots+B_n=0 \\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ Nv+N_2v_2+N_3v_3+\ldots+N_n=0 \end{array}\right\}$$

and the solutions are denoted by

$$m'v=a_1;\ m'v_2=a_2;\ \ldots m'v_{n-1}=a_{n-1};$$

we get the relations

$$\left.\begin{array}{l} m=Aa_1+A_2a_2+A_3a_3+\ldots+A_{n-1}a_{n-1}+A_nm'; \\ a=Pa_1+P_2a_2+P_3a_3+\ldots+P_{n-1}a_{n-1}+P_nm'; \end{array}\right\}$$

out of which flow all the rules for the *genesis* of Eliminants, and the application of them to solve linear eqq. of any degree.

In adapting the theory to the proof of elementary propositions, as, in forming the *Product* of two Eliminants, the paper urged the utility of the principle, that every Eliminant is *a linear function of any one of its columns*, and also, *of any one of its rows*;—which principle may often be so applied as to show by inspection, *à priori*, that certain constituents are excluded from this and that function, and thus enable us to obtain its value by assuming arbitrary values for such constituents. It deprecated (at least for elementary uses) the notations used by Mr. Spottiswoode* and others, not only as involving needless novelty to learners, but because no page can be broad enough to afford to write

$$(1, 2)(1, 1)'+(2, 2)(1, 2)'+(3, 2)(1, 3)' \text{ instead of } \mathrm{B}\mathrm{X}+b\mathrm{Y}+\beta\mathrm{Z},$$

and because accents, so related, are hard to see in a full page, and the general aspect of every element is so like that of every other element, that the fatigue of reading soon becomes confusing and intolerable.

2. But the main topic of the paper was to advocate the use of Eliminants in Geometry of three dimensions, especially in every systematic treatise on Surfaces of the Second Degree. Various illustrations and results were given, which the writer believed to be new; on which account, some of them may be briefly noticed here.

Problem. "To find the length of a perpendicular ρ, dropt from a given point $(a\,b\,c)$, on to a given plane $lx+my+nz+p=0$; when the axes are oblique, and the cosines of the angles $(xy)(xz)(yz)$ are *given*; viz.$=$D, E, F."

Result. Take G and H to represent the eliminants

$$\mathrm{G}=\begin{vmatrix} 1 & \mathrm{D} & \mathrm{E} \\ \mathrm{D} & 1 & \mathrm{F} \\ \mathrm{E} & \mathrm{F} & 1 \end{vmatrix} \text{ and } \mathrm{H}=\begin{vmatrix} 1 & \mathrm{D} & \mathrm{E}-l \\ \mathrm{D} & 1 & \mathrm{F}-m \\ \mathrm{E} & \mathrm{F} & 1-n \\ l & m & n \quad 0 \end{vmatrix};$$

then ρ is known from the eq.

$$\rho\sqrt{\mathrm{H}}=(la+mb+nc+p)\sqrt{\mathrm{G}}.$$

When ρ is given, this eq. determines the relations between $l\,m\,n\,p$, which are the test, that the plane may touch a sphere given in position.

* It may be right to state, that Mr. Newman opened the paper by a grateful and honourable recognition of Mr. Spottiswoode's labours.

Problem. To analyse the forms assumed by the locus of the general eq.

$$Ax^2+By^2+Cz^2+2A_2x+2B_2y+2C_2z$$
$$+2Dxy+2Exz+2Fyz+G=0 \text{ (axes oblique).}$$

Result. Let $V=\begin{vmatrix} A & D & E \\ D & B & F \\ E & F & C \end{vmatrix}$ and $W=\begin{vmatrix} A & D & E & A_2 \\ D & B & F & B_2 \\ E & F & C & C_2 \\ A_2 & B_2 & C_2 & G \end{vmatrix}$; then in the common treatises (only without this notation) it is shown that when V is finite, the surface (if real) has a centre. It is here added, that when W is negative, the curvature is everywhere towards the same side of the tangent plane; when W vanishes, the tangent plane coincides with the surface in one straight line; but when W is positive, the surface is *cut* by the tangent plane in two intersecting straight lines, and the curvature bends partly towards one side of the tangent plane, partly towards the other.

Hence it appears that we have different sorts of surfaces, by combining $V=0$ or $V=$finite, with $W=0$ or $W=$positive, or $W=$negative.

The locus is *imaginary*, if W is >0, A and B finite, $CG-C_2^2>0$, and $C\begin{vmatrix} A & E & A_2 \\ E & C & C_2 \\ A_2 & C_2 & G \end{vmatrix}>0$.

The locus is *degenerate*, if of ABC one at least (as C) be finite, and if $V=0$, $\begin{vmatrix} A & E & A_2 \\ E & C & C_2 \\ A_2 & C_2 & G \end{vmatrix}=0$, $\begin{vmatrix} B & F & B_2 \\ F & C & C_2 \\ B_2 & C_2 & G \end{vmatrix}=0$: or if A B C all vanish, and if at the same time $D=0$, and $E:F:C_2=2A_2:2B_2:G$.

Problem. To investigate the nature of the plane intersections of the surface.

Result. If the cutting plane be $lx+my+nz+p=0$, the section is a hyperbola, parabola or ellipse, according as $\begin{vmatrix} A & D & E & l \\ D & B & F & m \\ E & F & C & n \\ l & m & n & o \end{vmatrix}$ is positive, zero, or negative.

The intersection *degenerates*, if $\begin{vmatrix} A & D & E & A_2 & l \\ D & B & F & B_2 & m \\ E & F & C & C_2 & n \\ A_2 & B_2 & C_2 & G & p \\ l & m & n & p & o \end{vmatrix} = 0.$

In a non-centric surface, where $V=0$, we readily find that the former of these eliminants has the same sign as (D^2-AB); and consequently, that non-centric surfaces cannot have sections of opposite species. It also appears, that to determine in a non-centric surface the parabolic sections, we must take $l\,m\,n$ such as to verify one of the three eqq.

$$\begin{vmatrix} A & D & E \\ D & B & F \\ l & m & n \end{vmatrix} = 0, \quad \begin{vmatrix} A & D & E \\ l & m & n \\ E & F & C \end{vmatrix} = 0, \quad \begin{vmatrix} l & m & n \\ D & B & F \\ E & F & C \end{vmatrix} = 0.$$

Problem. To determine the circular sections, when they exist.

Result. Take the larger question, of ascertaining when two surfaces of the second degree intersect in a plane curve. Denote the coefficients of the second surface by accents. Put $\alpha = A\rho - A'$; $\beta = B\rho - B'$; $\gamma = C\rho - C'$; &c. and determine ρ by the eq.

$$\begin{vmatrix} \alpha & \delta & \epsilon \\ \delta & \beta & \phi \\ \epsilon & \phi & \gamma \end{vmatrix} = 0;$$

which involves ρ in the third degree.

Then $l\,m\,n$ will be determined (when the surds are real) by the proportion

$$l : m : n = \sqrt{(\epsilon^2 - \alpha\gamma)} + \epsilon : \sqrt{(\phi^2 - \beta\gamma)} + \phi : \gamma.$$

To apply this to the problem of circular sections, it is only necessary to suppose the second surface to be a sphere.

The surface becomes one of Revolution, if (with oblique axes) either system of three eqq. is fulfilled:

$$\begin{cases} (1) \quad \alpha\beta = \delta^2, \quad \alpha\gamma = \epsilon^2, \quad \beta\gamma = \phi^2, \\ (2) \quad \alpha\phi = \delta\epsilon, \quad \beta\epsilon = \phi\delta, \quad \gamma\delta = \epsilon\phi. \end{cases}$$

If out of each triplet we eliminate ρ^2 and ρ, (for it seems easiest to treat these as independent variables,) the result is two eqq. (expressible by eliminants), which are the two general conditions for a surface of revolution.

Problem. To find the system of rectangular conjugates. This of course is cardinal, and is treated everywhere: but is made far easier by Eliminants, as follows. Let us inquire after *that diameter, common to two given concentric surfaces, which shall have its conjugate planes the same for both.*

Take the centre for the origin, and $x=mz$, $y=nz$ for the common diameter sought. Then the central planes conjugate to it in the two surfaces are

$$\left.\begin{array}{l}(Am+Dn+E)x+(Dm+Bn+F)y+(Em+Fn+C)z=0\\(A'm+D'n+E')x+(D'm+B'n+F')y+(E'm+F'n+C')z=0.\end{array}\right\}$$

To identify these two planes, let

$$\frac{Am+Dn+E}{A'm+D'n+E'}=\frac{Dm+Bn+F}{D'm+B'n+F'}=\frac{Em+Fn+C}{E'm+F'n+C'}=\frac{1}{\rho},$$

or

$$\alpha m+\delta n+\epsilon=\delta m+\beta n+\phi=\epsilon m+\phi n+\gamma=0.$$

Eliminate m, n, and you find that ρ is to be determined by the very same eq. as in the preceding; and since its eq. is of the third degree, it has always one real value.

Next, let the second surface be a sphere, and you find *at least one* diameter of the first surface *perpendicular* to its conjugate plane. Make this diameter the axis of x, and take for the axes of y and z the two *principal* diameters of the section in the conjugate plane. Then $D=0$, $E=0$, $F=0$; so that the general eq. is reduced to $Ax^2+By^2+Cz^2+G=0$. Moreover, the system of axes is now rectangular: hence the axis of y, and that of z, equally with that of x, are each perpendicular to its conjugate plane, and the eq. for ρ must have three real roots, corresponding to these three axes.

We might similarly investigate "the conditions of contact for two concentric surfaces;" which, when one of them is a sphere, gives the cubic whose roots are a^2, b^2, c^2, principal axes of an Ellipsoid.

Problem. To discuss the results of *Tangential Co-ordinates.* [This expression is employed as by Dr. James Booth in an original tract on the subject.]

Put
$$\left.\begin{array}{l}P=Ax+Dy+Ez+A_2\\Q=Dx+By+Fz+B_2\end{array}\right|\begin{array}{l}R=Ex+Fy+Cz+C_2\\S=A_2x+B_2y+C_2z+G\end{array}$$

Then $Px+Qy+Rz+S=0$ is the eq. to the surface, and $Px'+Qy'+Rz'+S=0$ is the eq. to the tangent plane at (xyz). Hence if

$x'y'z'$ are the three tangential co-ordinates (or intercepts cut from the co-ordinate axes by the tangent plane) we have $Px'+S=0$, $Qy'+S=0$, $Rz'+S=0$. Let $\xi\,\eta\,z$ be the *reciprocals* of $x'\,y'\,z'$. Then $P+\xi S=0$, $Q+\eta S=0$, $R+\zeta S=0$; and the eq. to the surface becomes $\xi x+\eta y+\zeta z-1=0$. Restore for PQR their equivalents; then eliminating $xyzS$ you get

$$\begin{vmatrix} A & D & E & A_2 & \xi \\ D & B & F & B_2 & \eta \\ E & F & C & C_2 & \zeta \\ A_2 & B_2 & C_2 & G & -1 \\ \xi & \eta & \zeta & -1 & 0 \end{vmatrix} =0;$$

general eq. to the surface, with axes oblique.

If the last eq. (developed) be represented by

$$a\xi^2+b\eta^2+c\zeta^2+2a_2\xi+2b_2\eta+2c_2\zeta+2d\xi\eta+2e\xi\zeta+2f\eta\zeta+g=0,$$

it is not difficult to obtain a system of eqq. in which $abc\ldots\xi\eta\zeta$ play the same part, as just before did $ABC\ldots xyz$. Whence again we have

$$\begin{vmatrix} a & d & e & a_2 & x \\ d & b & f & b_2 & y \\ e & f & c & c_2 & z \\ a_2 & b_2 & c_2 & g & -1 \\ x & y & z & -1 & 0 \end{vmatrix} =0;$$

which is the *original* eq. of the surface under the form of an Eliminant.

The most arduous problems (as Dr. James Booth has shown) are often facilitated by these co-ordinates; but without Eliminants, the eqq. cannot be treated generally and simply.

The paper likewise contained the application of Eliminants to tangential co-ordinates in Curves of the Second Degree; and urged that eliminants ought to be introduced into the general treatment of these curves also, if only in order to accustom the learner to their use and gain uniformity of method. Thus, if the general eq. be

$$Ax^2+By^2+C+2Ex+2Fy+G=0,$$

then $V=0$ is the test of degeneracy.

March 26, 1857.

Major-General SABINE, R.A., Treas. and V.P., in the Chair.

The following communications were read :—

I. "On an Element of Strength in Beams subjected to Transverse Strain, named by the author 'The Resistance of Flexure.'" (Second Communication.) By WILLIAM HENRY BARLOW, Esq., F.R.S. Received March 12, 1857.

(Abstract.)

In his former paper on this subject the author pointed out the existence of an element of strength in beams when subjected to transverse strain,—the resistance of flexure—which had been omitted in the generally received theory ; and the object of the present experimental inquiry is to elucidate more clearly the general bearing of the subject, and determine more precisely the laws which govern this resistance.

The forms of beam employed in the experiments formerly described were only of two kinds—solid rectangular bars and open girders ; in the present experiments other forms have been used, namely, square bars broken on their sides, square bars broken on their angles, round bars, beams of the I section broken with the flanges horizontal, and similar beams broken with the flanges vertical ⊢⊣.

The results of these experiments are exhibited in Tables, together with those of the former series ; and the author employs them, in the first place, to test the accuracy of the existing theory, by comparing the resistance of the outer fibres or particles of each of the forms of beam, calculated on that theory, with the actual tensile strength of the metal as obtained by direct experiment. From this comparison applied to the different forms of beam, it would follow that the resistance at the outer fibre varies from 25,271 lbs. to 53,966 lbs., while the tensile strength of the metal, obtained by experiments on direct tension, averages only 18,750 lbs. ; and the dis-

crepancy and variation will be found to arise from the received theory not taking into account the resistance consequent on the molecular disturbance accompanying curvature.

In his former paper the author gave a formula by which the difference between the tensile strength and the apparent resistance at the outer fibre could be computed, approximatively, in solid rectangular beams and open girders; and he now proposes to trace the operation of the resistance of flexure, considered as a separate element of strength, and to show its effect, in each of the forms of section above indicated. Observing that the usual supposition of only two resistances in a beam, tension and compression, fails to account either for the strength, or for the visible changes of figure which take place under transverse strain, he proceeds to discuss the effects involved in such change of figure, and thence arrives at the following conclusions applicable to the resistance of flexure :—

1. That it is a resistance acting in addition to the direct extension and to compression.

2. That it is evenly distributed over the surface, and consequently (within the limits of its operation) its points of action will be at the centres of gravity of the half-section.

3. That this uniform resistance is due to the lateral cohesion of the adjacent surfaces of the fibres or particles, and to the elastic reaction which thus ensues between the portions of a beam unequally strained.

4. That it is proportional to and varies with the inequality of strain, as between the fibres or particles nearest the neutral axis and those most remote.

Formulæ are then given, according to these principles, exhibiting the relation between the straining and resisting forces in the several forms of section experimented on, as resulting from the joint effect of the resistances of tension, compression and flexure. The application of these formulæ to the actual experiments yields a series of equations with numerical coefficients, in which, were the metal of uniform strength, the tensile strength f, and the resistance of flexure ϕ, would be constant quantities, and their value might be obtained from any two of the equations; but as the strength varies even in castings of the same dimensions, and as a reduction of strength per unit of section takes place when the thickness is increased, the values

of f and ϕ will necessarily vary, and can only be ascertained in each experiment by first establishing the ratio they bear to each other. For this purpose the first ten experiments are used, in all of which the metal was from $\frac{3}{4}$ to 1 inch in thickness, and its mean tensile strength ascertained by direct experiment to be 18,750 lbs. per inch. The resulting mean value of ϕ is=16,573 lbs., and the ratio of f to ϕ as 1 to ·847.

By using results obtained by Prof. Hodgkinson on the breaking weight of inch bars of ten different descriptions of iron, where the tensile strength was ascertained by direct experiment, it would appear that the ratio between the resistance of tension and the resistance of flexure varies in different qualities of metal, an inference which seems to be confirmed by other experiments on rectangular bars given in the Report of the Commissioners on the application of iron to railway structures. The mean result, however, accords nearly with that of the author's experiments, and gives the ratio of f to ϕ as 1 to ·853. Hence, according to these data, the resistance to flexure, computed as a force evenly distributed over the section, is almost nine-tenths of the tensile resistance.

This ratio of the values of f and ϕ being applied to the equations resulting from the several experiments, gives the tensile strength of the metal as derived from each form of section, and the results, though not perfectly regular, are found to be within the limits of the variation exhibited by the metal as shown by the experiments on direct tension in the former paper. Classified and condensed, these results are as follows:—

The mean tensile strength as obtained from

The open girders, is	18,282
The solid rectangular bar of 2 inches sectional area	17,971
The inch bars—square and round, and square broken diagonally	19,616
The bars of 4 inches sectional area, square and round, and square broken diagonally	16,800
The compound sections in which the metal was $\frac{1}{2}$ inch thick	19,701

Having thus found that his formulæ, when applied to his own experiments, gave consistent and satisfactory results, the author next

tested them by other known experiments, and especially refers to those by Major Wade on the transverse strength of square and round bars of cast iron of different qualities, related in the "Reports on the Strength and other Properties of Metals for Cannon," presented to the United States Government by the Officers of the Ordnance Department. The unit of strength, as computed by Major Wade from these experiments, came out uniformly much higher in the round than in the square bars of the same kind of iron, whence he was led to doubt the correctness of the formula employed; but the author shows that when his formula is used, which includes the resistance of flexure, the discrepancy referred to disappears, and the tensile resistance, whether obtained for the round or the square bars, agrees very nearly with that derived from the experiments on direct tension under like circumstances.

As to the ratio between the resistance of flexure and the tensile resistance, it is remarked that, were the metal homogeneous, the former resistance would probably be precisely equal to the latter, instead of bearing the ratio of nine-tenths, as found by experiment; but the ratio evidently varies in different qualities of metal; and accordingly from Major Wade's experiments, it appears that with the same metal subjected to different modes of casting, an increase of transverse strength may accompany a decrease in the tensile resistance.

Respecting the limit of action of the resistance of flexure, the author observes, that in all the simple solid sections, the points of action are evidently the centres of gravity of the half-section; while in the compound sections it is necessary to compute the centre rib and flanges as for two separate beams in which the resistance of flexure is different, and has its point of action at the centre of gravity of the separate portions. It would appear that the elastic reaction developes this resistance to the full extent when the section is such that a straight line may be drawn from every point at the outer portion to every point at the neutral axis within the section; but that if the form of section is such that straight lines drawn from the outer fibres or particles to the neutral axis fall without the section, then it must be treated as two separate beams, each having that amount of resistance of flexure due to the depth of the metal contained in it.

The last section of the paper is devoted to the consideration of the resistance of flexure in wrought iron; and experiments are first

given to determine the position of the neutral axis, from which it is found to be at the centre of gravity of the section, as in cast iron; so that the action is the same in both materials, except as to the amount of the extensions and compressions with a given strain; and the formulæ given for cast iron will also apply to wrought iron. As wrought iron yields by bending and not by fracture, the relative value of f and ϕ are not so easily ascertained; moreover the ultimate compressive strain which wrought iron can sustain is little more than half its ultimate tensile strength; nevertheless the force required to overcome the elasticity of the material is nearly the same, whether applied as a compressive or tensile strain; the difference being, that the force which overcomes elasticity when applied as a compressive strain leads to the destruction or distortion of the material, while, in the case of the tensile strain, the elasticity may be overcome long before the material yields by absolute rupture.

A statement is given of the results of experiments made by Professor Barlow, in 1837, to show the weights which overcome the elasticity of the metal when applied transversely as compared with the weight necessary to produce the same result when applied by direct tension, and from these it is concluded that the resistance of flexure in wrought iron, considered as a force acting evenly over the surface, is nearly equal to one-half of the tensile resistance.

In an Appendix to this paper, by Professor Barlow (read at the following meeting), the preceding principles are applied to beams and rafters of non-symmetrical section.

With this view, the case of the double-flanged girder with unequal flanges is selected and discussed, and formulæ deduced, which are then tested by comparison with the results of experiments by Prof. Hodgkinson, published in the 'Manchester Memoirs;' a selection being made of those in which the girders differed most from each other in section, dimensions, and bearing-distance. The chief particulars of these experiments are given, with diagrams showing the forms of sections, and the values as obtained from the formulæ are stated. The value of the direct tensile strength of cast iron thus derived, falls between the limits of 1400 and 1700.

In the Reports of the Commissioners of Inquiry into the "Application of Iron to Railway Structures," are given the results of about fifty experiments on the direct tensile resistance of one-inch square

cast-iron bars, under the direction of Professor Hodgkinson. The bars consisted of seventeen different kinds of iron, each set of bars being of the like quality and manufacture; and in several of these sets, which might have been expected to yield the same results, the difference is fully as great as in the cases here exhibited. From this fact an inference may be drawn in favour of the general applicability of the principles developed in the foregoing pages to cast-iron beams and girders of every variety of section.

II. "On the Theory of the Gyroscope." By the Rev. WILLIAM COOK, M.A. Communicated by Professor A. WILLIAMSON, F.R.S. Received February 13, 1857.

(Abstract.)

The explanation of the movements of the Gyroscope (as well as its mathematical theory) is founded on the principle enunciated in the two following verbal formulæ.

I. When a particle is made to move $\left\{\begin{array}{l}\text{towards}\\\text{from}\end{array}\right\}$ a plane by any applied force, but in consequence of its connexion with some rigid body on the same side of the plane, loses some of its momentum in a direction perpendicular to the plane; all the momentum so lost is imparted to the rigid body, which is consequently impelled $\left\{\begin{array}{l}\text{towards}\\\text{from}\end{array}\right\}$ the plane.

II. When a particle is made to move $\left\{\begin{array}{l}\text{towards}\\\text{from}\end{array}\right\}$ a plane by any applied force, but in consequence of its connexion with some rigid body on the same side of the plane, receives an extra momentum in a direction perpendicular to the plane; all the momentum so gained is taken from the rigid body, which is consequently impelled $\left\{\begin{array}{l}\text{from}\\\text{towards}\end{array}\right\}$ the plane.

The mass of the disc of the gyroscope is supposed to be compressed uniformly into the circumference of a circle of given radius (r), and to revolve round an axis with a given uniform angular velocity (ω). To facilitate the arithmetical computation of the for-

mulæ, masses are represented by weights; so that any effective accelerating force f is supposed to be due to a pressure P acting on a mass W, and their relation expressed thus, $f=\frac{Pg}{W}$.

The mass of any arc of the circle is denoted by $\frac{cr\theta}{l}$; θ being the angle at the centre, and c the mass of a given length l of the circumference. The terms of all the formulæ are thus made homogeneous.

The centre of gravity of the disc, axle, and the ring which carries the pivots of the axle is fixed, and the whole is moveable about that centre in any manner, subject to the condition that the line of the pivots of the ring is always horizontal, unless when detached from the stand. Let this straight line of the pivots be denoted by AB, the common centre of the disc and ring by O, the extremities of the axle by N and S; and $ON=a$.

Let M denote the place of a particle of the disc, its position being determined by the angle AOM (θ), and let M′ be another point in the disc indefinitely near to M, but more remote from A, the direction in which the disc will presently be supposed to revolve being AMM′B.

A given force F is applied at N perpendicular to the plane ANBS, so that the disc may describe an angle ϕ round AB in the time t; whereby the points M and M′ describe the two arcs $MP=y$ and $M'P'=y'$ simultaneously. Suppose the circumference of the circle AMB to be divided into four quadrants, commencing at A, where $y=O$, and corresponding with the four ranges of value of θ through each of four right angles; suppose M and M′ to be in the first quadrant, so that y' is greater than y; then if the disc be supposed to revolve, a particle at M is carried from the line MP to the line M′P′, so as to acquire an increase of velocity from the plane AMM′ independently of the force F, and consequently (by the first of the two verbal formulæ) all the momentum so acquired by the particle is lost to the disc, ring, &c., which are thus impelled as by a force in the direction PM or P′M′, so as to oppose the rotation imparted by F, but to impart another round O in the direction ANB in the plane of the ring; *i. e.* in a plane perpendicular to that in which F acts. A force having the same tendency is found, by

means of one or the other of the two verbal formulæ, in the other three quadrants, and thus every particle (dm) of the disc contributes to the same effect. This effect is due to the difference of the velocities $\frac{dy}{dt}$ and $\frac{dy'}{dt}$ at P and P', or to the momentum $\left(\frac{dy'}{dt}-\frac{dy}{dt}\right)dm$ lost or gained by the particle dm in the time dt.

The value of $\frac{dy}{dt}$ is obtained from the equation $y=r\phi\sin\theta$, making both ϕ and θ to vary; but the value of $\frac{dy'}{dt}$ is obtained from that of $\frac{dy}{dt}$ by making θ only to vary. It is thus shown that

$$\left(\frac{dy'}{dt}-\frac{dy}{dt}\right)dm=\left(\cos\theta\,.\,\frac{d\phi}{dt}-\omega\,.\,\phi\sin\theta\right)r\omega dt dm.$$

It is thence shown, by taking the moments about AB, and applying D'Alembert's principle, that

$$\left(\frac{d^2\phi}{dt^2}+\omega^2\phi\right)\int\sin^2\theta d\theta-\omega\frac{d\phi}{dt}\int\sin\theta\,.\cos\theta d\theta=\frac{\mathrm{F}ag}{cr^3},$$

the integrals applying to θ only, and between the limits O and 2π; *i. e.* to all the particles of the disc simultaneously and independently of ϕ or t. From this is obtained the result

$$\phi=\frac{4\mathrm{F}ag}{\mathrm{W}r^2\omega^2}\,.\,\sin^2\left(\frac{\omega t}{2}\right);$$

W being the weight of the disc.

This value being periodical, and ranging between the limits O and the maximum $\frac{4\mathrm{F}ag}{\mathrm{W}r^2\omega^2}$, shows that the disc makes oscillations which are of less extent and duration, as the spinning of the disc is more rapid; *i. e.* as ω^2 is made greater compared with $\frac{\mathrm{F}}{\mathrm{W}}$; and thus if F denotes a small weight (such as is usually supplied with the apparatus by the makers), the extent of the oscillation becomes insensible. This formula, applied to the apparatus with which the experiments were made, gives the theoretical maximum of ϕ about 18 minutes of a degree. It is evident that when F represents a weight, it should be replaced in the differential equation by F $\cos\phi$, but the result practically coincides with that actually obtained when F is not excessive.

That these oscillations must exist will be evident, when it is considered that the gyroscope, with the weight attached and the disc not spinning, becomes an ordinary pendulum: the effect of the spinning being to disturb its oscillations, and to lessen their extent to an unlimited amount, whenever the spinning of the disc is sufficiently rapid.

The preceding investigations, as well as the experiments, show that whenever a force is applied to the axis of a revolving disc, more or less of the momentum due to this force is converted into a momentum of rotation parallel to a plane which is perpendicular to that in which the force acts.

April 2, 1857.

The LORD WROTTESLEY, President, in the Chair.

The following communications were read:—

I. "Researches on Silica." By Colonel Philip Yorke, F.R.S. Received March 25, 1857.

(Abstract.)

This communication is principally devoted to an attempt to determine the formula of silica, and to the relation of some remarkable results obtained in this research. After giving some account of the grounds on which the three different formulas now in use among chemists (viz. SiO_3, SiO_2, and SiO) had been advocated, the author proceeds to state, that it appeared to him that the direct method which had been followed by Rose deserved the preference. This method consists in determining the quantity of carbonic acid which is displaced from excess of an alkaline carbonate in fusion, by a given

weight of silica. The number 22 being the equivalent for carbonic acid on the hydrogen scale, the equivalent of silicic acid is obtained by the proportion $x = \frac{22 \times \text{weight of silica used}}{\text{weight of carbonic acid expelled}}$.

Four experiments are detailed, made with carbonate of potash, which give as a mean result the number 30·7 for the equivalent of silica. This agrees with the formula SiO_2, and nearly with the previous results of H. Rose. Then follow seven experiments made in a like manner with carbonate of soda, which give as a mean result the number 21·3 as the equivalent of silicic acid—a number agreeing nearly with half that represented by the formula SiO_3, or $\frac{45 \cdot 2}{2}$.

Some experiments are then related, which go to show that the increased loss resulting with carbonate of soda could not be caused by the action of heat alone.—The author had next recourse to carbonate of lithia, and obtained as the mean result of four experiments with this substance, agreeing well together, the number 14·99—a number which accords very closely with the formula SiO. These different numbers, obtained with silica, led the author to inquire whether any other body acting as an acid produces similar results with the fused carbonates of potash and soda. With this view, experiments mere made with dry sulphate of magnesia, as a substitute for sulphuric acid, with bi-borate of soda, for boracic acid, with alumina and sesquioxide of iron. Of these bodies only boracic acid gave results similar to those obtained with silicic acid. The other substances all gave the equivalent numbers usually assigned to them, equally with the carbonates of potash and soda.

Directing his attention then to determine whether the equivalent of silicic acid could be found in other volatile acids than the carbonic, the author relates some experiments made with the hydrates of potash and soda, but he explains that there are circumstances which render it much more difficult to obtain accordant numbers with these bodies than with the carbonates.

Six experiments made with hydrate of potash gave as mean result the same number as that obtained with the carbonate, viz. 30·8. But with hydrate of soda the mean of three experiments gave the number 17·2 as the equivalent of silicic acid,—a result approaching that previously obtained with carbonate of lithia.

A silicate of soda was formed by fusing together silica and carbonate of soda, in proportions indicated by the previous experiments—*i. e.* 23 silica to 54 carbonate of soda = 31 soda. The fused mass was crystallized. It was dissolved in water, and the solution evaporated *in vacuo* yielded a crystallized salt, which contained about 5 per cent. of carbonate of soda; when this, calculated as the ordinary 10-hydrated salt, was subtracted, the silicate agreed nearly with the formula $NaO . SiO_2 + 7HO$. A crystallized salt of like composition was obtained when hydrated silica was dissolved in a solution of caustic soda, the silica and soda being in the same proportions, *i. e.* 31 : 23. In these experiments it appears, therefore, that in the process of solution and crystallization a portion of soda is extruded. When exposed to a temperature of about 300° F., nearly all the water was driven off from these salts, less than 1 per cent. remaining.

The fused silicate of lithia in like manner, when treated with water, appeared to split up into different compounds.

After guarding himself from drawing any decided inference from the experiments recorded, the author concludes by observing that at present he can see no alternative but to admit of more than one equivalent for silicic acid (that is to say of more than one acid), the value of which is determined by circumstances,—such as the presence of water and the nature of the base to which it is presented. The existence of such different silicic acids has been already suggested by chemists on different grounds, particularly by Ebelman and Laurent, and lately by M. Fremy.

II. Appendix to the Paper of Mr. W. H. Barlow, "On an Element of Strength in Beams subjected to Transverse Strain," &c. By Peter Barlow, Esq., F.R.S. Received March 25, 1857.

A notice of this communication is given in the abstract of the paper to which it refers (see *antea*, p. 432).

III. "On the Application of Parabolic Trigonometry to the Investigation of the Properties of the Common Catenary." By the Rev. JAMES BOOTH, LL.D., F.R.S. Received March 19, 1857.

Some time ago, on the publication of a paper read by me last summer at Cheltenham before the Mathematical Section of the British Association on Parabolic Trigonometry and the Geometrical origin of Logarithms, Sir John Herschel called my attention to the analogy which exists between the equation of the common catenary referred to rectangular coordinates, and one of the principal formulæ of parabolic trigonometry. Since that time I have partially investigated the subject, and find, on a very cursory examination, that the most curious analogies exist between the properties of the parabola and those of the catenary,—that in general for every property of the former a corresponding one may be discovered for the latter. In this paper I cannot do more than give a mere outline of these investigations, but I hope at some future time, when less occupied with other avocations than at present, I may be permitted to resume the subject. I will only add, that the properties of this curve appear to be as inexhaustible as those of the circle or any other conic section.

II. The equations of the common catenary referred to rectangular coordinates are

$$y=\frac{m}{2}\left(e^{\frac{x}{m}}+e^{-\frac{x}{m}}\right), \qquad s=\frac{m}{2}\left(e^{\frac{x}{m}}-e^{-\frac{x}{m}}\right).$$

The point O may be called the focus, whose distance to the vertex A of the curve is $=m$.

It will simplify the investigations, without lessening their generality, if we assume the modulus or focal distance $m=1$.

Assume $2\sec\theta=e^{x}+e^{-x}$, $\quad 2\tan\theta=e^{x}-e^{-x}$ . . . . . . (1.)

Then $y=\sec\theta$, $\quad s=\tan\theta$ . . . . (2.)

Now if we make x', x'', x''', &c. successively equal to $2x$, $3x$, $4x$, &c., we shall have (see 'Parabolic Trigonometry,' No. XXVI.)

$$\begin{array}{ll}
y\;\;=\sec\;\theta & s\;\;=\tan\;\theta \\
y'\;=\sec(\theta\perp\theta) & s'\;=\tan(\theta\perp\theta) \\
y''=\sec(\theta\perp\theta\perp\theta) & s''=\tan(\theta\perp\theta\perp\theta) \\
y'''=\sec(\theta\perp\theta\perp\theta\perp\theta) & s'''=\tan(\theta\perp\theta\perp\theta\perp\theta)
\end{array}$$

Now ('Parabolic Trigonometry,' No. III.) it has been shown that

$$(\sec\theta+\tan\theta)^n=\sec(\theta\perp\theta\perp\text{to } n \text{ terms})+\tan(\theta\perp\theta\perp\text{to } n \text{ terms});$$

hence in the catenary we shall have

$$(y+s)^n=(y_{///}\ldots n+s_{///}\ldots n),$$

or *if two points on the catenary be assumed, the abscissa of one being* n *times that of the other, the* nth *power of the sum of the ordinate and arc of the latter will be equal to the ordinate and arc of the former.*

We may graphically exhibit with great simplicity the sum of a series of angles added together by the parabolic or logarithmic plus $\perp$.

Let a set of equidistant ordinates—for simplicity let the common interval be unity—meet the catenary in the points b, c, d, k, l, and then let the catenary be supposed to be stretched along the horizontal tangent passing through the vertex A. Let the points b, c, d, k, l, on the catenary in its free position, coincide with the points β, γ, δ, κ, λ, on the horizontal line when strained in that position, and as x or $A\beta$, $A\gamma$, $A\delta$, $A\kappa$, $A\lambda$ is successively equal to

$$m,\ 2m,\ 3m,\ 4m,\ \&\text{c. or to } 1,\ 2,\ 3,\ 4,\ \&\text{c. if } m=1,$$

we shall have

$$\begin{array}{ll} 2y\ \ =e^1+e^{-1} & s\ \ =e^1-e^{-1} \\ 2y_{/}\ =e^2+e^{-2} & s_{/}\ =e^2-e^{-2} \\ 2y_{//}=e^3+e^{-3} & s_{//}=e^3-e^{-3} \\ 2y_{///}=e^4+e^{-4} & s_{///}=e^4-e^{-4} \end{array}$$

Hence the angle $AO\beta$ or ε is such that

$$\sec\varepsilon+\tan\varepsilon=e,$$

$$AO\gamma \text{ such that, } \sec AO\gamma+\tan AO\gamma=e^2, \text{ or } AO\gamma=\varepsilon\perp\varepsilon$$
$$AO\delta \text{ such that, } \sec AO\delta+\tan AO\delta=e^3, \text{ or } AO\delta=\varepsilon\perp\varepsilon\perp\varepsilon;$$

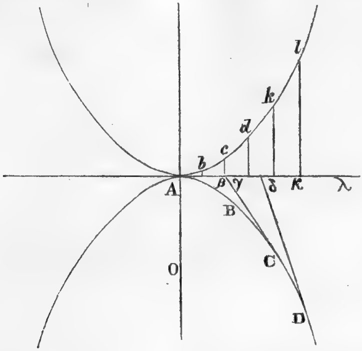
consequently if we draw lines from the focus O successively to the points β, γ, δ, κ, λ, the angles $AO\beta$, $AO\gamma$, $AO\delta$, $AO\kappa$, $AO\lambda$ will be the angles ε, $\varepsilon\perp\varepsilon$, $\varepsilon\perp\varepsilon\perp\varepsilon$, $\varepsilon\perp\varepsilon\perp\varepsilon\perp\varepsilon$.

This is one of the simplest graphical representations we can have of angles added together by the parabolic or logarithmic plus $\perp$.

Hence as successive multiples of an arc of a circle give successive arithmetical multiples of the corresponding angle at the centre, so successive multiples of a given abscissa give successive arcs of the

catenary which, extended along the vertical tangent, subtend at O successive parabolic multiples of the original angle.

Since the original interval was assumed equal to m, and as the arc



of the catenary is always longer than the abscissa which subtends it, or $A\beta > Ab$, it follows, as has been shown in 'Parabolic Trigonometry,' No. XXII., that $s > 45°$.

Since $\frac{dy}{dx} = \frac{1}{2}(e^x - e^{-x})$ we shall have $\frac{dy}{dx} = \tan\theta$, but as $\frac{dy}{dx}$ is the trigonometrical tangent of the angle which the linear tangent at the point (xy) makes with the axis of the abscissa, hence this other theorem:

Let a set of equidistant ordinates meet the catenary in the points b, c, d, k, l, *and at these points let tangents to the curve be drawn, they will be inclined to the axis of the abscissa by the angles* θ, $\theta \perp \theta$, $\theta \perp \theta \perp \theta$, $\theta \perp \theta \perp \theta \perp \theta$, &c., which is even a yet simpler geometrical representation than the preceding.

Hence also it evidently follows that as the limit of the angle which a tangent to the catenary makes with the axis of the abscissa is a right angle, the limit of the angle $\theta \perp \theta \perp \theta \perp \theta \perp \theta$, *ad infinitum*, is a right angle.

We have also this other theorem:

If with the point O *as focus, and* A *as vertex, we describe a parabola, and from the points* β, γ, δ, κ, λ *we draw tangents* βB, γC,

δD, κK, λL *to the parabola, the differences between these tangents and the corresponding parabolic arcs, namely,* AD, AC, AD, AK, AL, *will be* m, $2m$, $3m$, $4m$, *or*

$$AB-\beta B=m, \quad AC-\gamma C=2m, \quad AD-\delta D=3m, \quad AK-\kappa K=4m \text{ \&c.}$$

This is evident (see 'Parabolic Trigonometry,' No. XXVI.) for the angles $AO\beta=\varepsilon$, $AO\gamma=\varepsilon\perp\varepsilon$, $AO\delta=\varepsilon\perp\varepsilon\perp\varepsilon$, $AO\kappa=\varepsilon\perp\varepsilon\perp\varepsilon\perp\varepsilon$.

We may further extend these properties of the catenary. To simplify the expressions, let $Y\phi$ denote the ordinate of a point on the catenary at which the tangent makes the angle ϕ with the axis of X. Let $S\phi$ denote the arc measured from the lowest point, and let $X\phi$ signify the ordinate.

Then $$S\phi=\tan\phi, \qquad Y\phi=\sec\phi.$$

Now let x, $x_{\prime}$, $x_{\prime\prime}$ be the abscissæ of the three arcs whose tangents make the angles ϕ, χ, ω with the axis of x, and let the equation of condition be simply

$$x_{\prime\prime}=x+x_{\prime}.$$

Then we shall have the following relations between the corresponding arcs and ordinates of the catenary—

$$S\omega=S\phi\, Y\chi+S\chi Y\phi$$
$$Y\omega=Y\phi Y\chi+S\phi S\chi$$

when $$x_{\prime}=x$$
$$2S^2\phi=Y\omega-1$$
$$2Y^2\phi=Y\omega+1$$
$$Y\omega=Y^4\phi-S^4\phi.$$

Let there be four arcs of the catenary whose abscissæ x, $x_{\prime}$, $x_{\prime\prime}$, $x_{\prime\prime\prime}$ shall be connected by the following relation

$$x_{\prime\prime}=x+x_{\prime}$$
$$x_{\prime\prime\prime}=x_{\prime\prime}+x_{\prime\prime} \text{ or } x_{\prime\prime\prime}=x+x_{\prime}+x_{\prime\prime}.$$

Let $\bar{\omega}$, ϕ, χ, ψ be the corresponding angles made by the tangents to the extremities of the arcs $S\omega$, $S\phi$, $S\chi$, $S\psi$.

Then we shall have the following relations between the arcs and the ordinates—

$$S\bar{\omega}=S\phi Y\chi Y\psi+S\chi Y\phi Y\psi+S\psi Y\phi Y\chi+S\phi S\chi S\psi$$
$$Y\bar{\omega}=Y\phi Y\chi Y\psi+Y\phi S\chi S\psi+Y\chi S\phi S\psi+Y\psi S\phi S\chi.$$

Hence also

$$\frac{S\omega}{Y\phi Y\chi Y\psi}=\left(\frac{S\phi}{Y\phi}\right)+\left(\frac{S\chi}{Y\chi}\right)+\left(\frac{S\psi}{Y\psi}\right),$$

or the ratio of the fourth arc to the product of the ordinates of the three preceding arcs is equal to the sum of the ratios of each preceding arc to its ordinate.

We have also

$$\frac{Y\omega}{Y\phi Y\chi Y\psi}=1+\left(\frac{S\phi S\chi}{Y\phi Y\chi}\right)+\left(\frac{S\chi}{Y\chi}\frac{S\psi}{Y\psi}\right)+\left(\frac{S\psi}{Y\psi}\frac{S\phi}{Y\phi}\right).$$

Let $x=x_{\prime}=x_{\prime\prime}$, and $x_{\prime\prime\prime}=3x$,

then we shall have $S\omega=4S^3\phi+3S\phi$,

an equation which gives the relation between two arcs of the catenary, the abscissa of the one being equal to three times that of the other.

When one abscissa is double of the other, the arcs are related by the equation $2YS=S_{\prime}$.

Since $$\sin^2\phi=\frac{\sec(\phi\perp\phi)-1}{\sec(\phi\perp\phi)+1},$$

and $\sin\phi=\frac{S}{Y}$, we shall have

$$\frac{S^2}{Y^2}=\frac{Y_{\prime}-1}{Y_{\prime}+1},$$

an equation which enables us to calculate $Y_{\prime}$ when we know Y, since $S^2=Y^2-1$. Thus the catenary may be constructed by points.

Let $s, y, s_{\prime}y_{\prime}, s_{\prime\prime}y_{\prime\prime}, s_{\prime\prime\prime}y_{\prime\prime\prime}$ be four arcs and corresponding ordinates of a catenary, whose abscissæ are connected by the equation

$$x_{\prime\prime\prime}=x_{\prime\prime}+x_{\prime}+x,$$

then we shall have

$$\frac{s_{\prime\prime\prime}}{y_{\prime\prime\prime}}=\frac{\frac{s}{y}+\frac{s_{\prime}}{y_{\prime}}+\frac{s_{\prime\prime}}{y_{\prime\prime}}+\frac{ss_{\prime}s_{\prime\prime}}{yy_{\prime}y_{\prime\prime}},}{1+\frac{ss_{\prime}}{yy_{\prime}}+\frac{s_{\prime}s_{\prime\prime}}{y_{\prime}y_{\prime\prime}}+\frac{s.s_{\prime\prime}}{yy_{\prime\prime}}.}$$

The Society then adjourned over the Easter holidays to Thursday, April 23.

April 23, 1857.

The LORD WROTTESLEY, President, in the Chair.

The following communication was read:—

"On the Magnetic Induction of Crystals." By Professor JULIUS PLÜCKER, of Bonn, For. Memb. R.S., Hon. M.R.I., &c. Received March 26, 1857.

[Abstract.]

The author commences by referring to his discovery of the peculiar action of magnets on crystalline bodies, and to the researches to which he was thereby led. With reference to the form in which he enunciated the law regulating the action of a magnet on a uniaxal crystal—that the optic axis is attracted or repelled by the poles of the magnet—he disclaims any intention of assigning a physical cause to the phenomenon, or doing anything more than expressing the results of observation, which are *as if* such a force existed. In the case of crystals of a more complicated character, he was led, in the first instance, to assume the existence of two magnetic axes, possessing a similar character as to attraction and repulsion with the one axis of optically uniaxal crystals. But finding that the proposed law did not hold when the crystal was examined in all directions, and not solely along peculiar axes, he abandoned, nearly two years ago, a hypothesis respecting which serious doubts had arisen long before. For the hypothesis of one or two axes acted upon by the magnet, he substituted another similar hypothesis. In the case of uniaxal crystals he now conceived an ellipsoid of revolution, consisting of an amorphous paramagnetic or diamagnetic substance, and having within the crystal its principal axis coincident with the principal crystallographic axis. It is easy to verify that both crystal and ellipsoid, the poles of the magnet not being too near each other,

will be directed between them in exactly the same way. In the generalization, an ellipsoid with three unequal axes, having a determinate direction in the crystal, must be substituted for the ellipsoid of revolution. In this hypothesis too, two "magnetic axes" are met with, that is, according to the new definition, directions which possess, in common with the single crystallographic axis of uniaxal crystals, the property that if the crystal be suspended so that either of these axes is vertical, and the body is at liberty to turn freely round it, no extraordinary magnetic action is exhibited, but the crystal behaves like an amorphous substance.

According to observation, a crystal under favourable circumstances is directed in the same way as the smallest of its fragments. Hence, according to the new hypothesis, each of its particles may be regarded as acted on like an amorphous ellipsoid. But such an amorphous molecular ellipsoid, when influenced by a magnetic pole at a finite distance, will be directed like an ellipsoid of finite dimensions under the influence of an infinitely distant pole. Here Poisson's theory presented itself for the verification of the hypothetical conclusions and their consequences, to which the author had been led by considerations of a different kind. This verification had the most complete success. But before proceeding to it, it was found necessary to confirm Poisson's theory itself (or rather the results following from it), with respect to an ellipsoid of finite dimensions influenced by an infinitely distant pole. By means of a beautiful theorem lately published by Professor Beer, by which the results relating to the influenced ellipsoid are simply and elegantly expressed by means of an auxiliary ellipsoid, the author was enabled to deduce immediately the analytical expressions. These were afterwards compared with experiment, by observations made on two carefully worked ellipsoids of soft iron, executed by M. Fessel of Cologne.

The results thus obtained from theory, and verified by experiment, with reference to an amorphous ellipsoid, were compared with the results obtained from the observation of crystals, and manifested a complete agreement. According to this theory, the magnetic induction within a crystal is, like the elasticity of the luminiferous ether, determined by means of an auxiliary ellipsoid. As there are three rectangular axes of optical elasticity, so there are three principal axes of magnetic induction, characterized by the property that if a

crystal be suspended along any one of them, the two others set, one axially, and the other equatorially. As there are two optic axes, situated in the plane of the axes of greatest and least elasticity, so there are two magnetic axes, characterized by the property already mentioned.

Among crystals, the author selected for special examination red ferrocyanide of iron, sulphate of zinc, and formicate of copper. The first is paramagnetic, the second diamagnetic, and in both cases the principal axes of magnetic induction are determined by the planes of crystalline symmetry. The setting of elongated prisms, as well as of long cylinders and short cylinders or circular plates, cut in various selected directions from the crystals, is described in detail. The use of both cylinders and circular plates, cut with their axes in the same direction, obviated any objection which might be raised attributing the setting to the external form, since, so far as was due to mere form, a cylinder and a circular plate would set with their axes in rectangular directions.

Formicate of copper differs from the former crystals in having but one plane of crystalline symmetry, and accordingly in having but one principal axis of magnetic induction determined by the crystalline form. The existence of three principal magnetic axes, having the property already mentioned, was demonstrated experimentally, and the directions of those two which were not determined by the crystalline form, were ascertained by experiment. In this crystal the axes of greatest and least induction, and consequently the magnetic axes, lie in the plane of symmetry; and the existence of two magnetic axes was demonstrated, and their positions were determined.

In conclusion, the author gives a list of crystals, classified according to their paramagnetic or diamagnetic characters, and the order of magnitude of the magnetic inductions in the direction of their principal axes. He also remarks that some crystals, of which instances are given, though belonging according to their form to the biaxal class, have two of their principal magnetic inductions so nearly equal that they cannot be distinguished from magnetically uniaxal crystals; while others, though not belonging to the tesseral system, have all their principal inductions so nearly equal that they cannot be distinguished from amorphous substances.

April 30, 1857.

The LORD WROTTESLEY, President, in the Chair.

The following communication was read:—

"Inquiries into the Quantity of Air inspired throughout the Day and Night, and under the influence of Exercise, Food, Medicine, Temperature, &c." By EDWARD SMITH, M.D., LL.B., L.R.C.P., Assist. Physician to the Hospital for Consumption and Diseases of the Chest, Brompton. Communicated by Dr. CARPENTER, F.R.S. Received March 19, 1857.

(Abstract.)

This communication consists of three parts, and contains the results of 1200 series of observations.

In the first part is contained the results of three investigations as to the quantity of air breathed during the whole of the 24 hours, and registered every 5, 15, and 30 minutes. The second part describes the influence of posture, various kinds of exercise, physical agents, and different articles of food and medicine, on the quantity of air breathed, and on the frequency of the respiration and pulse. The third part is devoted to certain inquiries in relation to the temperature of the body.

The author was himself the subject of all the investigations. He is thirty-eight years of age, six feet in height, healthy and strong, and with a vital capacity of the lungs of 280 cubic inches. The instrument employed was Glover's patent dry gas meter, of improved manufacture, and arranged to register from 1 to 1,000,000 cub. ins. Its action was reversed so as to measure inspiration. An ori-nasal mouth-piece was employed, and it was connected with the spirometer by vulcanized caoutchouc tubing, and could be fastened upon the head by elastic straps.

The determination of the quantity of air inspired in the 24 hours was effected by three inquiries. 1st, by using the spirometer whilst

in the quiet sitting posture during five minutes at the commencement of each quarter of an hour from 5 A.M. to $6\frac{1}{2}$ P.M., and of each half-hour from $6\frac{1}{2}$ P.M. to $3\frac{1}{2}$ A.M. Exercise was taken in the intervals, and food was eaten at $8\frac{1}{2}$ A.M., 1, $5\frac{1}{2}$, and $8\frac{1}{2}$ P.M. 2ndly, by using the spirometer without intermission during the whole of the 24 hours, except at intervals amounting collectively to 40 minutes. The posture, exercise, and periods of sleep were noticed and recorded, and the quantity of air inspired was read off at the end of every five minutes. 3rdly, by an inquiry similar to the second, but continued through the night only, from $6\frac{3}{4}$ P.M. to $5\frac{3}{4}$ A.M.; the quantity being recorded every quarter of an hour. The first inquiry was made on July 7 and 8, 1856, the second on January 19 and 20, 1857, and the third on January 21, 1857.

The maximum influence of various agents was determined both by the quantity of air inspired taken absolutely, and also relatively to that recorded immediately before the influence in question was exerted. The inquiries were made before breakfast, or at periods distant at least three hours from the last meal, so as to avoid the disturbing influence of food. Exertion, variation of posture, mental inquietude, change of temperature, &c., were avoided during the continuance of each inquiry, and hence the observations were never continued beyond a period of two hours. The quantity of air was determined during periods of five minutes at a time, twice during the first quarter of an hour, and once at the commencement of each succeeding quarter of an hour. The results were averaged per minute.

In all the above-mentioned inquiries the rate of respiration and pulsation and the temperature of the wet and dry bulbs with the barometric pressure were recorded.

The temperature of the breath was determined by the aid of an instrument consisting of a boxwood tube $\frac{3}{4}$ of an inch in diameter and $1\frac{1}{2}$ inch in length, through the sides of which and at a right angle to it, a small thermometer was inserted and the bulb exposed freely to the exhaled breath. A valve was placed at the distal end which permitted the exit of the breath, but prevented the entrance of air, whilst near to the end which was placed between the lips when in use, a valve was fixed which moved in both directions. The bulb of the thermometer was thus enclosed in a small chamber.

The paper concludes with a summary of the principal results ob-

tained and a series of deductions, applicable especially to the solution or elucidation of hygienic questions. From the former the following facts are extracted:—

The total quantity of air inspired in 24 hours (allowance being made for intervals amounting altogether to 40 minutes, during which it was not recorded) was 711,060 cub. ins.; or an average of 29,627 cub. ins. per hour and 493·6 per minute. The quantity was much less during the night than during the day. There was an increase as the morning advanced and a decrease at about $8^h\ 30'$ P.M., but most suddenly at about 11 P.M. During the day the quantity increased immediately after a meal, and then subsided before the next meal; but in every instance it rose again immediately before a meal. The rate of frequency of respiration generally corresponded with the quantity, but the extremes of the day and night rates were greater. The period of greatest parallelism was between tea and supper. An increase was occasioned by one meal only, namely breakfast. The average depth of respiration was 26·5 cub. ins., with a minimum of 18·1 cub. ins. in the night, and a maximum of 32·2 cub. ins. at $1^h\ 30'$ P.M. The mean rate of the pulse was 76 per minute, the minimum at $3^h\ 30'$ A.M., the maximum at $8^h\ 45'$ A.M.; the difference being more than one-third of the minimum rate.

Sleep came on in two of the series of continuous observations, and the time of its occurrence was also that of the lowest quantities of air inspired.

The amount of breathing was greater in the standing than in the sitting posture, and greater sitting than lying. It was increased by riding on horseback, according to the pace, also by riding in or upon an omnibus. In railway travelling the increase was greater in a second- than in a first-class carriage, and greatest in the third-class and on the engine. An increase was also produced by rowing, swimming, walking, running, carrying weights, ascending and descending steps, and the labour of the tread-wheel; and in several of these cases the rate of increase was determined for different degrees of exertion used. Reading aloud and singing, and the movement recommended by Dr. Hall for restoring suspended respiration, increased the quantity; bending forwards whilst sitting, lessened it.

The quantity of inspired air was increased by exposure to the heat and light of the sun, and lessened in darkness. Increase and decrease of artificial heat produced corresponding effects; and the

depth of respiration was greatly increased by great heat. An increase in quantity was caused also by cold bathing, and sponging, and the cold shower-bath; by breakfast, dinner, and tea—when tea actually was taken, but when coffee was substituted there was a decrease. Supper of bread and milk also caused a decrease. Milk by itself or with suet caused an increase.

An increase was obtained with the following articles of diet, viz. eggs, beef-steak, jelly, white bread (home-made), oatmeal, potatoes, sugar, tea, rum (1 oz.). The following caused a decrease, viz. butter, fat of beef, olive oil, cod-liver oil, arrow-root, brandy (1 oz. to $1\frac{3}{4}$ oz.), and kirchenwasser. Ether ($\frac{1}{2}$ drachm) increased the quantity and depth of inspiration. A decrease in quantity was caused by sp. ammon. co. (ʒiss), sp. ammon. fœt. (ʒiss), tincture of opium (20 ♏), morphia ($\frac{1}{6}$ and $\frac{1}{8}$ gr.), tartarized antimony ($\frac{1}{2}$ gr.), and chloride of sodium.

Carbonate of ammonia (15 grains) caused a small increase at first and then a small decrease; febrifuge medicines had a like effect. Chloroform (25 ♏ and ʒss), by the stomach, varied the quantity from an average increase of 28 cub. ins. to an average decrease of 20 cub. ins. per minute; with a maximum increase of 63 cub. ins. per minute. Chloric ether (ʒss) also varied the quantity, but there was an average increase of 17 cub. ins. per minute, and of 1·8 per minute, in the rate; whilst the pulse fell on the average 1·7 per min. Chloroform, by inhalation (to just short of unconsciousness), lowered the quantity a little during the inhalation, and more so afterwards. The rate was unchanged, but the pulse fell, on an average, 1·7 per min. Amylene similarly administered and to the same degree, increased the quantity during inhalation 60 cub. ins. per min., but afterwards decreased it to 100 cub. ins. per min. less than during the inhalation. The rate of respiration was unchanged: the pulse fell 6 per min. at the end of the observation.

Digitalis (infusion ℥i) varied the quantity, increasing it at first and then decreasing it. The rate of inspiration was unaffected, whilst that of pulsation somewhat increased.

The paper is accompanied by tables of numerical statements, and by diagrams exhibiting the results in a series of curves.

The President announced that the next Meeting of the Society, on the 7th of May, would be held in Burlington House.

May 7, 1857.

IN BURLINGTON HOUSE.

The LORD WROTTESLEY, President, in the Chair.

The following list of Candidates recommended by the Council for election into the Society was read, in accordance with the Statutes:—

Lionel Smith Beale, Esq.
George Boole, Esq.
George Bowdler Buckton, Esq.
Thomas Davidson, Esq.
George Grote, Esq.
Rowland Hill, Esq.
The Rev. Thomas Kirkman.
William Marcet, M.D.
John Marshall, Esq.
Andrew Smith, M.D.
Robert Angus Smith, Esq.
Charles Piazzi Smyth, Esq.
Henry Clifton Sorby, Esq.
John Welsh, Esq.
Joseph Whitworth, Esq.

The following communications were read:—

I. "On the Plasticity of Ice, as manifested in Glaciers." By James Thomson, A.M., C.E. Belfast. Communicated by Professor W. Thomson, F.R.S. Received April 1, 1857.

The object of this communication is to lay before the Royal Society a theory which I have to propose for explaining the plasticity of ice at the freezing point, which is shown by observations by Professor James Forbes, and which is the principle of his Theory of Glaciers.

This speculation occurred to me mainly in or about the year 1848. I was led to it from a previous theoretical deduction at which I had arrived, namely, that the freezing point of water, or the melting point of ice, must vary with the pressure to which the water or the

ice is subjected, the temperature of freezing or melting being lowered as the pressure is increased. My theory on that subject is to be found in a paper by me, entitled "Theoretical Considerations on the Effect of Pressure in Lowering the Freezing Point of Water," published in the Transactions of the Royal Society of Edinburgh, vol. xiv. part 5, 1849 *. It is there inferred that the lowering of the freezing point, for one additional atmosphere of pressure, must be ·0075° centigrade; and that if the pressure above one atmosphere be denoted in atmospheres as units by n, the lowering of the freezing point, denoted in degrees centigrade by t, will be expressed by the formula

$$t = \cdot 0075\, n.$$

The phenomena which I there predicted, in anticipation of direct observations, were afterwards fully established by experiments made by my brother, Professor William Thomson, and described in a paper by him, published in the Proceedings of the Royal Society of Edinburgh (Feb. 1850) under the title, "The Effect of Pressure in lowering the Freezing Point of Water experimentally demonstrated †."

The principle of the lowering of the freezing point by pressure being laid down as a basis, I now proceed to offer my explanation, derived from it, of the plasticity of ice at the freezing point as follows:—

If to a mass of ice at 0° centigrade, which may be supposed at the outset to be slightly porous, and to contain small quantities of liquid water diffused through its substance, forces tending to change its form be applied, whatever portions of it may thereby be subjected to compression will instantly have their melting point lowered so as to be below their existing temperature of 0° cent. Melting of those portions will therefore set in throughout their substance, and this will be accompanied by a fall of temperature in them on account of the cold evolved in the liquefaction. The liquefied portions being subjected to squeezing of the compressed mass in which they originate, will spread themselves out through the pores of the general

* The paper here referred to is also to be found in the Cambridge and Dublin Mathematical Journal for November 1850 (vol. v. p. 248), where it was republished with some slight alterations made by myself.

† The paper by Prof. William Thomson, here referred to, is also to be found republished in the Philosophical Magazine for August 1850.

mass, by dispersion from the regions of greatest to those of least fluid pressure. Thus the fluid pressure is relieved in those portions in which the compression and liquefaction of the ice had set in, accompanied by the lowering of temperature. On the removal of this cause of liquidity—the fluid pressure, namely,—the cold which had been evolved in the compressed parts of the ice and water, freezes the water again in new positions, and thus a change of form, or plastic yielding of the mass of ice to the applied pressures, has occurred. The newly-formed ice is at first free from the stress of the applied forces, but the yielding of one part always leaves some other part exposed to the pressure, and that, in its turn, acts in like manner; and, on the whole, a continual succession goes on of pressures being applied to particular parts—liquefaction in those parts—dispersion of the water so produced, in such directions as will relieve its pressure,—and recongelation, by the cold previously evolved, of the water on its being relieved from this pressure. Thus the parts recongealed after having been melted must, in their turn, through the yielding of other parts, receive pressures from the applied forces, thereby to be again liquefied, and to enter again on a similar cycle of operations. The succession of these processes must continue as long as the external forces tending to change of form remain applied to the mass of porous ice permeated by minute quantities of water.

Postscript received 22nd April, 1857.

It will be observed that in the course of the foregoing communication, I have supposed the ice under consideration to be porous, and to contain small quantities of liquid water diffused through its substance. Porosity and permeation by liquid water are generally understood, from the results of observations, and from numerous other reasons, to be normal conditions of glacier ice. It is not, however, necessary for the purposes of my explanation of the plasticity of ice at the freezing point, that the ice should be at the outset in this condition; for, even if we commence with the consideration of a mass of ice perfectly free from porosity, and free from particles of liquid water diffused through its substance, and if we suppose it to be kept in an atmosphere at or above 0° centigrade, then, as soon as pressure is applied to it, pores occupied by liquid water must in-

stantly be formed in the compressed parts in accordance with the fundamental principle of the explanation which I have propounded —the lowering, namely, of the freezing or melting point by pressure, and the fact that ice cannot exist at 0° cent. under a pressure exceeding that of the atmosphere. I would also wish to make it distinctly understood that no part of the ice, even if supposed at the outset to be solid or free from porosity, can resist being permeated by the water squeezed against it from such parts as may be directly subjected to the pressure, because the very fact of that water being forced against any portions of the ice supposed to be solid will instantly subject them to pressure, and so will cause melting to set in throughout their substance, thereby reducing them immediately to the porous condition.

Thus it is a matter of indifference as to whether we commence with the supposition of a mass of porous or of solid ice.

II. "On the part which the Silicates of the Alkalies may play in the Metamorphism of Rocks." By T. STERRY HUNT, Esq., of the Geological Survey of Canada. Communicated by THOMAS GRAHAM, Esq., F.R.S., Master of the Mint. Received March 30, 1857.

In my last communication to the Royal Society on the Metamorphic Silurian Strata of Canada, I endeavoured to show, from the results of analyses of the altered and unaltered rocks, that it is the reaction between the siliceous matters and the carbonates of lime, magnesia, and iron of the sedimentary deposits, which has given rise to the serpentines, talcs, pyroxenites, chlorites, and garnet rocks of the formation. I then cited the observation of Bischof that silica, even in the form of pulverized quartz, slowly decomposes these carbonates at a temperature of 212° F., with evolution of carbonic acid; the same author mentions that a solution of carbonate of soda has the power of dissolving quartz under similar conditions*. Desiring to verify these observations, I have since made the following experiments.

* Bischof's Chem. and Phys. Geology, Eng. Edition, vol. i. p. 7.

Colourless crystalline quartz was ignited, finely pulverized, and then boiled for an hour with a solution of its weight of perfectly pure carbonate of soda; the amount of silica thus dissolved was 1·5 per cent. of the quartz, but on repeating the treatment of the same quartz with a second portion of the carbonate, only ·35 per cent. was dissolved. The object of this process was to remove any soluble silica, and the quartz thus purified was employed for the following experiments, which were performed in a vessel of platinum.

I. 1000 parts of quartz and 200 of carbonate of soda were boiled with water for ten hours, and the mixture was several times evaporated to dryness, and exposed for a few minutes to a temperature of about 300° F. The amount of silica taken into solution was 12 parts.

II. A hydro-carbonate of magnesia was prepared by mingling boiling solutions of sulphate of magnesia and carbonate of potash, the latter in excess; the precipitate was washed by boiling with successive portions of water. 1000 parts of quartz were mixed with about as much of this magnesian carbonate and boiled as above for ten hours. An excess of hydrochloric acid was then added, the whole evaporated to dryness, and the magnesian salt washed out with dilute acid. The residue was then boiled for a few minutes with carbonate of soda, and gave 33 parts of soluble silica.

III. A mixture of 1000 parts of quartz, 200 of carbonate of soda with water, and an excess of carbonate of magnesia was boiled for ten hours, and the residue, treated as in the last experiment, gave 148 parts of soluble silica. The alkaline liquid contained a little magnesia but no silica in solution. That the soluble silica was really combined with magnesia was shown by boiling the insoluble mixture with sal-ammoniac, which, dissolving the carbonate, left a large amount of magnesia with the silica. This silicate was readily decomposed by hydrochloric acid, the greater part of the silica separating in a pulverulent form.

The third experiment was suggested by some observations on the reactions of silicate of soda with earthy carbonates. Kuhlmann has remarked the power of carbonate of lime to abstract the silica from a boiling solution of soluble glass*, and it is known that alumina

* Comptes Rendus de l'Acad. des Sciences, Dec. 3rd and Dec. 10th, 1855, where will be found many important observations on the alkaline silicates.

exerts a similar action. I have found that when artificial carbonate of magnesia in excess is boiled with a solution of silicate of soda, the latter is completely decomposed with the formation of carbonate of soda, and a silicate of magnesia which gelatinizes with acids ; and I have long since described this reaction in the evaporation of alkaline mineral waters*. This mutual decomposition of carbonate of magnesia and silicate of soda, conjoined with the power of carbonate of soda to dissolve silica, leads to a curious result. If we boil for some hours a mixture of ignited silica, obtained from the decomposition of a silicate by an acid (and consequently readily soluble in an alkaline carbonate), with a small portion of carbonate of soda and an excess of hydrocarbonate of magnesia, we obtain a dense powder which contains all the silica united with magnesia, and may be boiled with carbonate of soda and sal-ammoniac without decomposition. It is obvious from the above experiments that similar results may be obtained with quartz, although the process is much slower ; it would doubtless be accelerated under pressure at a somewhat elevated temperature, which would enhance the solvent power of the alkaline carbonate.

Silicates of potash and soda are everywhere present in sedimentary rocks, where decomposing felspathic materials are seldom wanting, and these salts in the presence of a mixture of quartz and earthy carbonates, aided by a gentle heat, will serve to effect a union of the quartz with the earthy bases, eliminating carbonic acid. A small amount of alkali may thus, like a leaven, continue its operation indefinitely and change the character of a great mass of sedimentary rock. Such a process is not only a possible but a necessary result under the circumstances supposed, and we cannot, I think, doubt that alkaline silicates play a very important part in the metamorphism of sedimentary rocks, which are composed for the most part of earthy carbonates, with siliceous, aluminous, and felspathic materials†.

The direct action between the carbonates and silica must necessarily be limited by their mutual insolubility, and by the protecting influence of the first-formed portions of earthy silicate ; but with the solvent action of a small portion of alkali which is changed from

* Reports of the Geol. Survey of Canada, 1851–53–54.

† It is well known that small portions of alkalies are seldom or never wanting in the earthy silicates, such as serpentine, talc, pyroxene, asbestus, epidote, idocrase, and even beryl and corundum. See the memoir of Kuhlmann already cited.

silicate to carbonate, and then back again to silicate, the only limit to the process would be the satisfying of the mutual affinities of the silica and the basic oxyds present.

III. "On the Comparison of Transcendents, with certain applications to the Theory of Definite Integrals." By GEORGE BOOLE, Esq., Professor of Mathematics in Queen's College, Cork. Communicated by Professor W. F. DONKIN, F.R.S. Received March 16, 1857.

(Abstract.)

The following objects are contemplated in this paper:—

1st. The demonstration of a fundamental theorem for the summation of integrals whose limits are determined by the roots of an algebraic equation.

2ndly. The application of that theorem to the comparison of algebraical transcendents.

3rdly. Its application to the comparison of functional transcendents, *i. e.* of transcendents in the differential expression of which an arbitrary functional sign is involved.

4thly. Certain extensions of the theory of definite integrals both single and multiple, founded upon the results of the application last mentioned.

In the expression of the fundamental theorem for the summation of integrals, the author introduces a symbol, Θ, similar in its definition to the symbol employed by Cauchy in the Calculus of Residues, but involving an additional element. The interpretation of this symbol is not arbitrary, but is suggested by the results of the investigation by which the theorem of summation is obtained. All the general theorems demonstrated in the memoir either involve this symbol in their expression, or are immediate consequences of theorems into the expression of which it enters.

The author directly applies his theorem of summation both to the solution of particular problems in the comparison of the algebraical transcendants, and to the deduction of general theorems. Of the

latter the most interesting, but not the most general, is a finite expression for the value of the sum

$$\Sigma\int\phi\psi^{\frac{m}{n}}dx,$$

where ϕ and ψ denote any rational functions of x; the equation by which the limits of the integrals are determined being of the form $\psi^{\frac{m}{n}}=\chi$, in which χ is also a rational function of x.

The forms of ϕ, ψ, and χ are quite unrestricted, except by the condition of rationality. Previous known theorems of the same class, such as Abel's, suppose ψ a polynomial and specify the form of ϕ. In the author's result, the rational functions ϕ, ψ, and χ are not decomposed. In a subsequent part of the paper, after investigating a general theorem applicable to the summation of all transcendents which are irrational from containing under the sign of integration any function which can be expressed as a root of an equation whose coefficients are rational functions of x, he explains, by means of it, the cause of the peculiarity above noticed.

In the section on functional transcendents, a remarkable case presents itself in which the several integrals under the sign of summation, Σ, close up, if the expression may be allowed, into a single integral taken between the limits of negative and positive infinity. The result is an exceedingly general theorem of definite integration, by means of which it is demonstrated, that the evaluation of any definite integral of the form

$$\int_{-\infty}^{\infty}\phi(x)f\left(x-\frac{a_1}{x-\lambda_1}-\frac{a_2}{x-\lambda_2}\,..-\frac{a_n}{x-\lambda_n}\right)dx,$$

in which $\phi\,(x)$ is a rational function of x, and in which $a_1\ a_2\,..\,a_n$ are positive, and λ_1, $\lambda_2\,..\,\lambda_n$ are real, the number of those constants being immaterial, may be reduced to the evaluation of a definite integral of the form

$$\int_{-\infty}^{\infty}\psi(v)f(v)dv,$$

in which $\psi(v)$ is a rational function of v of the same order of complexity as the function $\phi(x)$. Two limited cases of this theorem are referred to as already known,—one due to Cauchy, the other published by the author some years ago.

The remainder of the paper is occupied with applications of the

above general theorem of definite integration. Of the Notes by which the paper is accompanied, the first discusses the connexion between the author's symbol and Cauchy's, and contains two theorems, one exhibiting the general solution of linear differential equations with constant coefficients, the other the general integral of rational fractions. Both these theorems involve in their expression the symbol Θ. The second Note is devoted to the interpretation of some theorems for the evaluation of multiple integrals, investigated in the closing section of the paper.

May 14, 1857.

General SABINE, R.A., Treas. and V.P., in the Chair.

The following communications were read:—

I. "On the Organization of the Brachiopoda." By ALBANY HANCOCK, Esq. Communicated by T. H. HUXLEY, Esq., F.R.S. Received April 24, 1857.

(Abstract.)

In the present memoir the author states at length, and fully illustrates by figures, the conclusions to which he has been led by a long series of researches into the anatomy of the Brachiopoda; investigations which have been conducted with a special reference to the discrepant opinions maintained by Prof. Owen and the older writers on the one hand, and by Prof. Huxley and himself on the other. Some of the points in dispute have already been discussed in a paper read before the British Association at Cheltenham, and in the present memoir the author not merely reiterates the statements which he then made, but gives a detailed account of the whole organization of the Brachiopoda based upon his dissections of the following species:—*Waldheimia australis*, *W. Cranium*, *Terebratulina caput-serpentis*, *Rhynchonella psittacea*, *Lingula anatina*, and another species of *Lingulu*.

The Brachiopoda are divisible into two groups, according as the valves of their shells are articulated or not. *Waldheimia* is the type of the former group, *Lingula* of the latter.

In the articulated forms there are usually three apertures opening into the pallial chamber; of these one is the mouth,—the other two are situated at the apices of the organs which have been described as "hearts." In *Rhynchonella,* where there are four such "pseudo-hearts," there are of course five apertures instead of three. In *Lingula,* which possesses a distinct anus, opening on the right side of the pallial cavity, the apertures into the cavity are four, viz. one oral, one anal, and two appertaining to the pseudo-hearts.

After a description of the general arrangement of the organs in the articulated and non-articulated Brachiopoda, an elaborate account of the various systems of organs is given.

The muscles of the Terebratulidæ are divisible according to their functions into two groups,—the adductors of the valves, and those which adjust the shell upon the pedicle. Of the former, or "valvular" muscles, there are three pairs,—the adductors, cardinals, and accessory cardinals of previous writers; which the author prefers to term *occlusors, divaricators,* and *accessory divaricators.* Of the latter there are likewise three pairs, the so-called dorsal and ventral pedicle muscles and the capsular muscle; these the author terms the *dorsal and ventral adjustors,* and the *peduncular* muscle. The attachment of the muscles in *Waldheimia australis* and their actions are particularly described. The peduncular (capsular) muscle is shown to be the continuation of the muscular fibres contained within the peduncle. In the other articulated Brachiopoda examined, the arrangement of the muscles is essentially the same, but interesting differences are observable even in closely allied species.

Thus, in a species differing but little from *Waldheimia australis,* and in *W. Cranium,* the divaricators and accessory divaricators are united. In *Waldheimia Cranium* and *Terebratulina caput-serpentis* the dorsal adjustor muscles are not attached to a hinge-plate, but are inserted into the valve itself. In *Rhynchonella psittacea* there is a pair of peduncular muscles. In *Lingula* there are six pairs of muscles, all of which have both extremities attached to the valves. They have been divided into adductors and sliding muscles, the latter again being subdivided into protractors and retractors; but the author, considering that no sliding motion takes place, regards the latter terms as improper, and gives a set of new names, of which a concordance with the older denominations is subjoined.

Names in use.	Names proposed.	Names of homologous muscles of articulate Brachiopoda.
Anterior retractors	Anterior occlusors	Anterior occlusors.
Anterior adductors......	Posterior occlusors.........	Posterior occlusors.
Posterior adductors	Divaricators	Accessory divaricators.
Central protractors......	Central adjustors	Ventral adjustors.
External protractors ...	External adjustors	
Posterior retractors ...	Posterior adjustors.........	Dorsal adjustors.
Capsular	Peduncular	Peduncular.
	Anterior parietals.	
	Posterior parietals.	

The author conceives that the valves are separated by the action of the divaricators, combined with that of the parietals; these muscles compressing the visceral cavity posteriorly, and thus driving its contents into the anterior portion. The antagonists of these are the occlusors; while the office of the adjustors appears rather to be to supply the place of a hinge, and to prevent anything like sliding of the valves one over the other.

The muscular fibres of *Lingula* are smooth and unstriated. In *Waldheimia* those of the posterior occlusors are strongly striated, but the rest of the muscles have smooth fibres. The arms, their attachment and minute structure are next fully described.

In *Waldheimia* the canals of the attached portions of the arms coalesce into a single wide tube, which lies externally between the produced and reflected crura of the calcareous loop, and is separated by a partition from a canal of corresponding size—the "brachial sinus,"—which also extends throughout the whole length of the produced and reflected crura, and is in fact a prolongation of the perivisceral chamber. The cirri are arranged in this and all the other Brachiopoda examined, in a double alternating series—not in a single row, as has hitherto been stated to be the case. The walls of the brachial canal are tolerably well supplied with delicate muscular fibres, which run diagonally round the tube, and are most strongly developed towards the sides, near the grooved ridge which supports the cirri. An indistinct band of exceedingly delicate longitudinal fibres may also be observed nearly opposite to it. The author has however completely failed to discover, either here or in *Rhynchonella*, anything like the double spiral arrangements of fibres described by

Prof. Owen, and believes that the latter observer has mistaken the blood-seinuss for muscles.

The author doubts whether the spiral coil can be unwound, and conceives that the muscular fibres described, are chiefly for the purpose of giving firm support to the grooved ridge on which the cirri and brachial fold are seated, and thus affording the complex muscular fibres which the ridge contains a better fulcrum whence to act upon the cirri.

In *Terebratulina caput-serpentis,* which possesses no calcareous loop, the pallial lobe connecting the produced and reflected portions of the arms is strengthened by calcareous spicula, which are so numerous as to preserve the shape of the part even when the animal basis is removed.

In *Lingula* the arms contain two canals; one, the anterior, being the equivalent of the single canal in *Rhynchonella,* and, like it, terminating at the side of the œsophagus in a blind sac. The posterior brachial canal probably communicates with the perivisceral cavity and exhibits a peculiar arrangement of muscles, by whose action perhaps the arm can be exserted.

In addition to those parts of the alimentary canal and its appendages which are already known in the articulated Brachiopoda, the author describes a short median gastro-parietal band arising from the upper surface of the stomach and passing upwards and backwards to the dorsal parietes a little in advance of the hinge-plate. With regard to the existence or absence of an anal aperture in the articulated Brachiopoda, the writer states: "I have made numerous dissections under a powerful doublet, and have removed the part and examined it with a microscope: I have filled the tube with fluid as the fingers of a glove with air, and by pressure have attempted to force a passage: I have tried injections; but have equally, on all occasions, failed to discover an outlet, and have only succeeded in demonstrating more and more clearly the cæcal nature of the terminal extremity of the alimentary canal. Therefore, how much soever it may be opposed to analogy and to authority, the fact must be recorded—there is no anal orifice in *Waldheimia, Terebratulina,* or in *Rhynchonella.*"

In *Lingula,* as in the articulated Brachiopoda, the first inflection of the intestine is towards the ventral surface, but the alimentary

canal eventually ends in the easily observable anus placed nearer the dorsal than the ventral surface, on the right side of the body. The rudimentary mesentery, and the lateral gastro-parietal and ilio-parietal bands of *Lingula* are described. There is no median gastro-parietal band. Fæcal matter rolled into round pellets is commonly observable in the intestine of *Lingula,* while no fæces are ever found in that of the articulated Brachiopoda.

The genitalia in the articulate Brachiopoda are developed between the two membranes of which the inner wall of the pallial sinuses in which they are contained is composed, and, thrusting the inner of the two membranes from the outer, form a prominent mass connected by a band with the inferior wall of the sinus. The genital artery runs along the upper or outer edge of the band, and the genitalia are developed round it.

In *Lingula* the reproductive organs are withdrawn from the mantle and lodged within the visceral chamber, forming four irregularly lobulated or branched masses, two above and two below the alimentary canal, so that they may be distinguished as dorsal and ventral genital masses. The dorsal ovaries are suspended by the ilio-parietal bands, and the ventral by the continuation of these bands along the free margins of the pseudo-hearts. In both cases the attachment is along the margins of the bands, which are related to the genitalia much in the same manner as the suspending membrane is to the genital bands in *Waldheimia*; and it would seem that in *Lingula* the reproductive organs are really developed between the two layers composing the ilio-parietal bands. The author adduces arguments to show that the *Lingulæ* are hermaphrodite, the testis being a reddish mass, which ramifies over the true ovary.

The ova probably make their way out by the so-called "hearts," which open by their apices into the pallial cavity, and by their patulous bases (the so-called auricles) into the perivisceral chamber, and are hence capable of performing the functions of oviducts. The author has assured himself of the constant presence of the apical aperture of the pseudo-heart in all Brachiopoda. As pointed out by Prof. Huxley, there are four of these pseudo-hearts in *Rhynchonella,* but only two were found in the other Brachiopoda examined.

The pseudo-hearts have nothing to do with the propulsion of the blood, a function which is performed chiefly by the pyriform vesicle

discovered by Prof. Huxley in *Waldheimia* and *Rhynchonella*, and which was found attached to the stomach in all the Brachiopoda examined. It is composed of two layers, the inner distinctly muscular, the outer transparent and homogeneous. Connected with this heart are vessels or blood-channels (particularly described in the Memoirs); the "venous canals," which open into it anteriorly, returning the blood conveyed by the posterior arterial channels into the system of peripheral sinuses originally described by Prof. Huxley.

Accessory "hearts" or pulsatile vesicles have been found in some of the articulated Brachiopoda; the mantle and the walls of the body are essentially composed of a plate of substance traversed by reticulated lacunæ, and lined upon each side with epithelium. After explaining at length the distribution of the lacunæ throughout the mantle, the sheath of the intestine, its bands, the genital folds, the arms, &c., the author proceeds to give the following sketch of the course of the circulation:—

"Having now gone over all that I have been able to ascertain with respect to the central and peripheral portions of the circulatory apparatus, and having also examined the lacunes and blood-canals of the brachial organs, it will not be difficult to follow the flow of the blood throughout its entire course in *Waldheimia*; and as it is in it, so will it be in all probability in all other Brachiopods.

"It has been shown that the heart is a simple, unilocular, pyriform vesicle, suspended from the dorsal aspect of the stomach, and projecting freely into the perivisceral chamber; that there is neither auricle nor pericardium,—unless the membrane which closely invests it can be so called,—that it is hardly more complex in structure than the pulsating vessel of the *Tunicata*; and that in *Lingula*, indeed, it scarcely at all differs from the heart of these lowly organized mollusks. This vesicle, or heart, propels the blood through four arterial trunks or channels, to the reproductive organs and mantle, and probably also to the alimentary tube, and is apparently assisted by four or more pulsating vesicles in connexion with these principal trunks. The blood thus conveyed by the genital or pallial arteries will escape by the lacunes in the membranes suspending the genitalia, into the plexus in the floor of the great pallial sinuses. Thence it will find its way into the outer lacunary system of the pallial lobes, and into that of the dorsal and ventral walls of the body, as well as into the

lacunes of the anterior parietes. Having saturated all these parts of the peripheral system, it will divide itself into two currents, one of which will set backwards in the direction of the membranous bands connecting the alimentary tube to the parietes, and will flow through their channels into the system of visceral lacunes, which encircle the alimentary canal within the sheath, and which probably carry blood to the liver. This current will also supply blood to the lacunes nourishing the muscles. The blood thus directed will reach the branchio-systemic vein, either by the great œsophageal lacunes, or through the foramina which penetrate the sides of the channel as it runs along the dorsal ridge of the stomach.

"The other blood-current will set forward in the direction of the base of the arms, and some of it will pass into these organs through their general system of lacunes; but the principal portion will be carried by the afferent brachial canal to the extensive plexus of lacunes in those parts, and will circulate, in the manner before pointed out, within the walls of the great brachial canal. The blood will then be drawn up one side of the cirri through the vessels—the afferent brachial arteries—originating in the great brachial plexus, and returning down the other, will be poured into the efferent brachial canal, and thus reach the lateral efferent sinuses at the root of the œsophagus. Thence it will enter the great œsophageal lacunes, and there meeting with the other current of returning blood from the visceral lacunes, will be carried to the heart by the branchio-systemic vein along the dorsal side of the stomach.

"Thus it is perceived that the blood finds its way back to the central organ in a mixed condition. That which is conveyed by the gastro-parietal and other bands will be imperfectly aërated, having only flowed through the pallial membranes, which must be looked upon as but accessory oxygenating agents. The arms undoubtedly perform the office of gills, and are true respiratory organs. The blood which circulates through them will consequently be returned in a perfectly aërated condition, to be mixed, however, with that in a less pure state from the visceral lacunes before it enters the heart. This mixed state of the blood is not by any means peculiar to these animals, for it obtains in many of even the higher mollusks."

The perivisceral cavity and the great pallial sinuses have no communication with the proper blood-vascular system, but are to be com-

pared to the atrium of the Ascidianida, and the water-chambers of the Cephalopoda and other mollusca. The pseudo-hearts enable the perivisceral cavity to communicate with the exterior, and convey away the genital, and probably the renal products. On this head the author says:—

"From the foregoing account of the circulatory apparatus, it would appear that the perivisceral chamber, and its various so-called vascular ramifications in the mantle, are not connected with the blood-system. This is no doubt a startling fact. I commenced the present investigation fully imbued with the opinion that these parts were blood reservoirs and channels, and I only relinquished it when it became no longer tenable. Step by step the points relied on had to be abandoned, until at length the full conviction was arrived at that I had been seeking to establish a fallacy. I have been unable to discover any communication between the true blood-system and the pseudo-vascular ramifications in the mantle or the perivisceral chamber. Injections were thrown into this chamber, but none of the fluid found its way into any part of the lacunary system. The pallial lobes were removed, and the great pallial sinuses distended to their fullest capacity, with exactly the same result; and it was not until the tissues were ruptured on applying great pressure, that a little of the injected matter was extravasated into the peripheral lacunes. The perivisceral chamber, then, and all its various ramifications, are in no way connected with the true blood-system."

The nervous system of the articulated Brachiopoda is described at length. Besides the principal subœsophageal ganglion, two minute enlargements are shown to exist upon the anterior part of the œsophageal commissure, and two small pyriform ganglia are described in connexion with the under part of the principal ganglion. The peripheral nerves are minutely traced out, and two peduncular nerves, not hitherto known to exist, are described. The author denies the existence of the so-called "circumpallial" nerves. He has been unable to detect the nervous centres in *Lingula*, and he is inclined to regard the cords, described as nerves in that genus by Prof. Owen, as blood-sinuses.

The author next makes some remarks on the structure of the shell, pointing out that in *Terebratulina caput-serpentis* there are two distinct layers, an external and an internal; and he then draws

attention to the important anatomical characters which separate the articulated Brachiopoda as a group from the inarticulate division.

In conclusion, the author draws a parallel between the Brachiopoda and the Polyzoa, demonstrating the close structural conformity between these two groups.

II. "On the Placenta of the Elephant." By Professor RICHARD OWEN, F.R.S. &c. Received April 1857.

(Abstract.)

In this paper the author gives a description of the fœtal membranes and placenta of the Indian Elephant. The chorion forms a transversely oblong sac about 2 feet 6 inches in long diameter, encompassed at its middle part by a placenta of an annular form, 2 feet 6 inches in circumference, from 3 inches to 5 inches in breadth, and from 1 inch to 2 inches in thickness; in structure resembling that of the annular or zonular placenta of the Hyrax and Cat. The part of this placenta which had been detached from the maternal portion occupied a narrow annular tract near the middle line of the outer surface. A thin brown deciduous layer was continued from the borders of the placenta for a distance varying from 1 to 3 inches upon the outer surface of the chorion. Flattened folds of a similar layer of substance, or false membrane, could be raised from some parts of the surface of the placenta; at other parts the substance formed irregular fibrous bands,—the fibres extending in the direction of the circumference of the placental ring. The outer surface of the chorion is for the most part smooth and even shining, but at each of the obtuse extremities of the sac there was a villous subcircular patch, between 2 and 3 inches in diameter, the villi being short and graniform, $\frac{1}{6}$th of a line in diameter or less. Thus the chief points of attachment of the chorion to the uterus are, at the equator by the annular placenta, and at each pole of the elongated sac by the subcircular villous patch. The umbilical cord was short and rather flattened: it was formed by two arterial and one venous trunks, and by the slender neck of the allantois, with the connecting cellular tissue and the covering of amnios: it measured about 6 inches in

length, before the division of the vascular trunks, and about 3 inches in circumference. The inner surface of the amnios is roughened by brownish hemispherical granules, from 1 line to $\frac{1}{10}$th of a line in size—commonly about $\frac{1}{2}$ a line; the outer surface is finely wrinkled, but smooth; the amnios is continued from the base of the umbilical cord upon the allantois, which is of considerable size, and is so interposed between the chorion and amnios as to prevent any part of the amnios attaining the inner surface of the placenta. The amnios consists of two layers: one is the granular layer, continued upon the inner or fœtal surface of the allantois, and thence upon the umbilical cord; the other is the smooth outer layer, continued upon the outer or chorional surface of the allantois, and thence upon the inner surface of the chorion. The allantois divides where the amnios begins to be reflected upon it into three sacculi; the disposition of these sacculi is described in detail. The chief peculiarity was the presence, upon the inner layer of the allantois, and chiefly upon the endochorionic vessels, of numerous flattened oval or subcircular bodies, varying in diameter from an inch to half a line: their tissue was compact, structureless, and of a grey colour. On dissecting some of the vessels over which these bodies were placed, the vessel was found to pass on the chorionic side of the body without undergoing any apparent change, the body being developed from the allantois, and from that part which forms the allantoic side of the sheath of the vessel. These bodies were most numerous near the placenta: their free surface was smooth, not villous like the cotyledons of the Ruminantia, from which they likewise differed in projecting inwards towards the cavity of the allantois. The most important modification of the vascular structures connecting the chorion with the uterus, in the Elephant, is their combination of two forms of the placenta, viz. the 'annular' and the 'diffused,' which have hitherto been supposed to characterize respectively distinct groups of the class Mammalia.

The author concludes by a comparison of the different known forms of the placenta, including those of the *Pteropus* or large frugivorous Bat, and of the Chimpanzee; and by remarks on the value of placentary characters in the classification of the Mammalia.

May 28, 1857.

The LORD WROTTESLEY, President, in the Chair.

The CROONIAN LECTURE was delivered by JAMES PAGET, Esq., F.R.S., "On the Cause of the Rhythmic Motion of the Heart," as follows:—

I have selected for the subject of my lecture, the cause of the rhythmic motion of the heart; guided to this choice, partly by the belief that the Croonian lecture must have some relation to muscular motion, and partly by the interest which I have acquired in the subject in the course of observations extended, though with many and long interruptions, over some ten or twelve years.

It is not necessary that I should enter on any consideration of the various opinions that have been entertained on the cause of the heart's peculiar motion. Let me first show what it is, and how it differs from the other motions in the same body, which are visible to the naked eye.

In a beheaded tortoise, or any other of the Amphibia, the muscles of the trunk and limbs are usually in perfect rest, unless disturbed: those in the head may act so as to produce a kind of gasping and swallowing movements, at distant and nearly regular intervals (an imperfect kind of rhythmic motion): the digestive and other mucous canals are at rest, or move with slow worm-like actions; but the heart maintains, with perfect regularity, the rhythmically alternate contractions and dilatations of its auricles and ventricle; its several movements being ordered, not only in the manner of their succession, but in rhythm, *i. e.* in the proportions of time which they severally occupy.

Why is this difference? To what may we refer as the cause of this apparently peculiar mode of muscular movement? In answer, let me show (for it is a condition of the Croonian foundation that experiments should be shown) that the cause of the rhythmic action is in the heart itself; not in any of the great nervous centres through which other muscular movements are excited, including those in the

separated head, which will cease at once on destruction of the medulla oblongata and brain.

The evidence of this is in the continued action of the heart after it is cut out of the body. It may remain at rest for a few minutes after the excision; but then, as if recovering from shock or fatigue, it again begins to act, and thus will continue for many hours acting as regularly as it did when its connexions were unbroken.

The time during which the action of the cut-out heart will be maintained is different in the several classes of the Vertebrata; but some such continuance may be observed in all, and in all the experiment is enough to prove that the rhythmic action does not depend either upon distant nervous organs, or upon the blood which naturally flows through the cavities of the heart; for as soon as the heart is cut out, its cavities are emptied and no blood flows through them.

Thus, then, for a first conclusion, we may be sure that the cause of the rhythmic action of the heart is something in the heart itself; and this, notwithstanding the variations of the rhythm, which may be produced by morbid or artificial states of organs far distant from the heart.

But the cause, whatever it be, is not equally in all parts of the heart; for when its parts are in certain manners separated, some continue to act rhythmically, and others cease to do so. If, for example, the cut-out heart be divided into two pieces, one comprising the auricles and the base of the ventricle, the other comprising the rest of the ventricle, the former will continue to act rhythmically, the latter will cease to do so, and no rhythmic action can be, by any means, excited in it. The piece of ventricle does not lose its power of motion, for if it be in any way stimulated, it contracts even vigorously; but it never contracts without such an external stimulus, and, when stimulated, it never contracts more than once for each stimulus. Without losing motility, it has lost all rhythmic power, and all appearance of spontaneous action. We can, indeed, make it imitate a rhythm by stimulating it at regular intervals, such as every two or three seconds; but in any experiment of this kind, it will presently appear how much sooner the motor force is exhausted by the artificially excited actions, than by the apparently equal actions of the natural rhythm.

Other sections of the heart, and experiments of other kinds, would show that the cause of the rhythmic action of the ventricle, and probably also of the auricles so long as they are associated with it, and not with the venous trunks, is something in and near the boundary-ring between the auricles and ventricle; for what remains connected with this ring, or even with a part of it in a longitudinally bisected heart, retains its rhythm, and what is disconnected from it loses rhythm.

An experiment related by Heidenhain *, seems to show more precisely where the source of the rhythmic action of the ventricle is seated. If the ventricle of a frog's heart be separated, and every part of the septum of the auricles be removed from its base, so that its cavity may be perfectly single; and if, then, it be set upright on a board with some blood in its cavity and around it, it will be found that as pieces are cut away from its upper border, so its pulsations become less and less frequent, till at length, when a zone of a certain depth has been removed, they cease altogether. The depth of the zone to be cut away may be nearly one-third of the length of the ventricle; and somewhere in this zone we must assume lies that on which the rhythmic action of the ventricle, when alone or with part of the auricles, depends.

But wherever may be precisely the sources or centres of the rhythmic action of the heart, or any of its parts (and most of these details, I think, have yet to be determined, even for the hearts of Amphibia, and much more for those of other orders), these experiments seem enough to prove that the rhythm does not depend on the properties of the muscular tissue alone or independently. If it were so, it would be highly improbable that certain portions of the heart being separated should lose rhythmic action, and others just like them, so far as their muscularity is concerned, should retain it.

This conclusion is confirmed by many experiments invented by Professor Stannius †, and often repeated by myself and others, with results varying little from those which he obtained.

Perhaps the most remarkable of them is, that if a ligature be tied tightly round the place of conflux of the great veins entering the

* Quoted in Canstatt's 'Jahresbericht' for 1855, p. 130, from his "Disquis. de nervis organisque centralibus ocrdis." Berlin, 8vo.

† Müller's 'Archiv,' 1852, p. 95.

auricles of a frog's or tortoise's heart, the rhythmic action immediately, or after a few beats, ceases, and is suspended for three or many more minutes, and then returns in the ventricle alone (I believe) at a much slower rate than it had before.

This result is not due to the stoppage of the flow of blood into the heart, for if the veins be tied separately at a distance from the auricles, the rhythm continues. Neither is it due to the loss or great diminution of mere motility; for if the heart thus brought to rest be stimulated, it acts once, or rarely more than once, its action travelling from the part stimulated over all its substance. The result seems due to an injury done to something beneath or behind the ligature, from or through which some influence would naturally be conducted from the conflux of the veins to the auricles, and thence to the ventricle; for in the other direction, *i. e.* in the trunks of the veins behind the ligature, even to a great distance from the auricle, rhythmic motion still continues, and is scarcely, if at all, changed. And by variously placing the ligature, it would be found that, wherever tied between the venous sinus and the auriculo-ventricular ring, all the parts behind it would retain, and all those before it would lose (at least, for a time), their rhythm.

This indication of the experiment (that the loss of rhythm, namely, depends on injury of something beneath or behind the ligature) is confirmed by another, whose whole import, however, I am quite unable to explain.

If, when a heart is thus brought to rest by a ligature tied round the conflux of the veins, another ligature be tied round the boundary ring of the auricles and ventricle, and including the bulbus arteriosus of the frog, or the two aortæ of the tortoise, the rhythmic action remains suspended in the auricles, but at once begins again in the ventricle, or, if it have not wholly ceased there, is much accelerated. We thus obtain a condition of the heart in which the great veins and the ventricle act rhythmically, though not with the same rhythm, but the auricles between them are at rest.

Now, in all these facts there appears sufficient evidence that the source of the rhythmic action of the heart is not in the muscular structure alone, or in its relation with the blood in the cavities, or in the vessels of the heart; for these things are not in any of the experiments more disturbed in one part of the heart than in another.

The nutritive relations of the muscular tissue and the blood are sufficient for the maintenauce of mere motility, but they are not sufficient for the maintenance of rhythmic motion; they can maintain the power of acting upon external stimulus, but cannot give spontaneity of action, or regulate the time or manner of acting.

It is interesting to observe in these experiments three different modes of action of the heart: (1) the truly rhythmic, in which the contractions of its several parts, or of some of them when separated, ensue spontaneously (*i. e.* without evident stimulus or change of external conditions), and observe a definite order and proportion of time; (2) such as may be excited by stimulus, which, beginning at the cavity stimulated (whichever it may be), are effected simultaneously, or with a scarcely appreciable difference of time, by all parts of the walls of that cavity; then follow, with a certain interval, in the other cavities; and then are not repeated in the same order, but give place to the true rhythmic action, which they may for once have prevented or inverted; (3) those which ensue on stimulus, when the heart or any of its parts is utterly exhausted, and which commence at the point stimulated, and thence slowly travel once to all other points connected with it by continuity of muscle.

Now, of these three modes of action, the last may be ascribed to mere motility, such as the intestines and the stomach have, and such as is maintained by the nutritive relations of the muscular substance and the blood in its vessels. The second may be due to the same, acting with more energy; or it may be ascribed, as by Bidder* and Rosenberger†, to the action of reflecting nervous centres in the ganglia of the heart. But what of the third, the rhythmic, which every experiment shows to be essentially different from both of these?

It is explicable, and all the experiments are consistent, on the belief (which many before me have entertained) that it depends on certain nervous centres in the nerve-ganglia of the heart; which centres, by spontaneous discharges of nerve-force, cause the muscular structures to contract. These centres (rhythmic centres as they have been called) being injured or hindered in their operation, the rhythm ceases, though the motor power is not lost; or these centres, being cut away with certain portions of the heart, the other portions cease to

* Müller's 'Archiv,' 1852, p. 163.

† De centris motuum cordis. Dorpat, 8vo, 1850.

have rhythmic motion, though still capable of repeated single actions when artificially stimulated.

Such nervous centres may exist among the numerous ganglia in the heart, and the indications of their position in the frog's heart given by the experiments, agree very well with the dissections by Bidder and Rosenberger. There is, indeed, nothing in the structure or aspect of the ganglia presumed to be rhythmic, which can make us sure that they can act as nervous centres of peculiar force, much less (if it were possible) that they must act rhythmically. But the assumption of such rhythmic nervous centres is justified, not only by its sufficiency as an hypothesis, and by its accordance with the existence of ganglia in the appropriate places in hearts, but by the analogy of the only other example of naturally rhythmic muscular movements in our own or any other mammalian economy,—I mean the respiratory movements. These are certainly determined in their rhythm by the medulla oblongata. It is true, they are greatly influenced as to their rate and force, though not as to their very existence, by external conditions, and are amenable in a measure to the power of the will; and the rate and force of their rhythm are much more dependent, than are those of the heart's, on the conditions of distant parts. Nevertheless, essentially and habitually, the medulla oblongata is not only the nervous centre, but the rhythmic nervous centre, of the respiratory movements. When a certain portion of it is destroyed, the proper respiratory rhythm ceases, and nothing can renew it, whatever stimulus or variety of external conditions be present. Any of the respiratory muscles may be again stimulated to contraction; some of them will again, and for a few times, contract repeatedly and at regular intervals, imitating the old rhythm*; but there is no longer any combination or timely succession of the movements of the many respiratory muscles, as for their proper purpose.

* Such surviving rhythmic movements of the respiratory muscles may, perhaps, be ascribed to the continuance of the rhythmic nutrition, of which mention is made in a later part of the lecture, and which, though not the primary cause of their rhythmic action, must coexist with it; but other than the respiratory muscles may thus for a time act rhythmically when dying, or after their nerves are divided. See especially the observations of M. Brown Séquard in his 'Experimental Researches.' The facts may be very difficult to explain, but are not inconsistent with what I offer as the explanation of the rhythmic action of the heart.

In like manner, it may be that the rhythmic movements of the lymphatic hearts in Amphibia depend on a part of the spinal cord as their nervous centre; but this is doubtful*; doubtful I mean, not whether there be a nervous centre for the rhythmic movements, but where it is seated. The case of the respiratory movements, however, is clear, and should be reckoned as of great weight in the argument for the nervous origin of the heart's action.

The experiments on the heart's action which I have shown, have been selected as the simplest and most significant. I pass by many more, and say concerning them only this,—that I believe there are none whose results are inconsistent with the belief that I have expressed as to the cause of the rhythmic action of the heart. There are some, of which the results are difficult to explain; but the difficulties relate to questions of nerve-physiology†, and do not affect the simple conclusion that the rhythmic action of the heart in the Vertebrata depends on the operation of nervous centres in the ganglia on or near the substance of the heart: I say, in the Vertebrata; for as yet we know (I believe) nothing of the origin of the corresponding movements in the Invertebrata, though we may well believe that in them, and especially in the lower among them, the rhythmic movements of any pulsating vessel, analogous to a heart, are independent of any nervous system. Such a difference in the two great groups would correspond with similar differences in nearly all other parts of their several economies. The more highly developed the nervous system is, the more are its operations influential in those of every other part and system; and, for the heart, we may suppose that the making-over of its rules of action to a proper nervous system is on purpose that it may the more quickly correspond and sympathize

* See the Experiments of Schiff, Mayer and Budge, in Canstatt for 1850, p. 126; and those of Heidenhain in the same, for 1855, p. 130.

† I refer, especially, to those on the pneumogastric nerves, of which the continued galvanic stimulus stops the heart's action. The phenomena observed in this and similar experiments appear to me very similar to those of *shocks*. As a violent shock of any kind may exhaust the power, or suspend the action, of the brain or spinal cord, so may a shock by violence or galvanic force similarly affect the power of the rhythmic nervous centres for the heart. And the general explanation of all may be, that the nutrition of a nervous centre, and thereby the maintenance of its power, requires rest, and that this rest cannot exist while nerves in relation with it are under irritation.

with other organs, with which, but for their connexion through the nervous system, it could only sympathize more slowly through the medium of the blood.

But, now, it will not have escaped you to question,—why are these nervous centres rhythmic in their action? granting all that has been said, why is it that these nervous centres accumulate and discharge nerve-force, as it would seem, not only spontaneously, but at time-regulated intervals? To put on them, rather than on the heart's muscles, the work of rhythm, is only to put the real difficulty of the matter a step further back.

This is very true; and I will add, that they who hold that the rhythmic, like the ordinary, action of muscles, cannot ensue without stimulus, are in the like predicament, at a step still further back; for how, it may be asked, is the stimulus (of the heart suppose) applied or generated at regular intervals? And the difficulty is not in the case of the heart alone, but in that of all the rhythmic muscular movements. For why, it may be asked, does the medulla oblongata rhythmically excite the respiratory muscles into action? and if it be answered that it is itself stimulated by venous blood, or by impressions on the pneumogastric and other centripetal nerves, then how does the venous blood, or any other substance, thus rhythmically and not constantly stimulate? Constant stimulus, or constant production of an excess of nervous or muscular force, might produce constant muscular action, whether continuous or disorderly, but could not alone maintain a regularly interrupted action, in which the lengths of time of action and of inaction are in definite proportion.

This shifting of the question as to the cause of rhythmic motion suggests that we should enter on a larger inquiry, and take notice of whatever organic processes are performed with rhythm; and this seems the more necessary, while we consider that rhythmic action is not tied to any particular structure, whether muscular or nervous, nor employed in only one or two purposes, as blood-movement or breathing; nor even limited to the animal kingdom: so that, really, the peculiarity we have to study is not one of force, or not one of force alone, but one of *time* as an element in the organic processes. No explanation of the rhythmic action of the heart, therefore, would be sufficient, which did not involve or appear consistent with some general law to which we may refer all other rhythmic organic pro-

cesses, that is, all such as are accomplished with time-regulated alternations, whether of motion or any other change.

Probably, the simplest example of rhythmic motions yet known is that detected by the acute researches of Professor Busk* in the *Volvox globator*. At a certain period of the development of this simplest vegetable organism, there appear, in each zoospore, or in the bands of protoplasm with which the zoospores are connected, vacuoles, spaces, or cavities, of about $\frac{1}{9000}$th of an inch in diameter, which contract with regular rhythm at intervals of from 38 to 41 seconds, quickly contracting and then more slowly dilating again.

The observations of Cohn†, published about a year later than those of Mr. Busk, but independent of them, discovered similar phenomena in *Gonium pectorale* and in *Chlamydomonas*, the vacuoles, like water-vesicles, contracting regularly at intervals of 40 to 45 seconds. The contractions and the dilatations occupy equal periods, as do those of our own heart-ventricles; and in *Gonium* he has found this singular fact, that when, as commonly happens, two vacuoles exist in one cell, their rhythms are alike and exactly alternate, each contracting once in about 40 seconds, and the contraction of each occurring at exactly mid-distance between two successive contractions of the other.

Here, then, we have examples of perfect, and even of compound, rhythmic contractions in vegetable organisms, in which we can have no suspicion of muscular structure, or nervous, or of stimulus (in any reasonable sense of the term), or, in short, of any one of those things which we are prone to regard as the mainsprings of rhythmic action in the heart.

The case of ciliary movements is as simple; the cilia having a perfect rhythm, so that their alternate and opposite movements are of definitely proportioned length; and that, where many move on the same surface, they all keep time precisely, by moving, not all simultaneously, but in time-regulated succession. Here, too, we have no trace of muscular or nervous structure, or of stimulus; yet there is a perfect rhythm in which even myriads of cilia keep time,

* Transactions of the Microscopical Society of London, May 21, 1852.

† Untersuchungen über die Entwickelungsgeschichte der mikroskopischen Algen und Pilze. Breslau, 4to, 1854.

although there is no evident connecting medium between them, nor, generally, any continuity of structure.

I cite these two instances—the ciliary motion and the contraction of the vacuoles or vesicles in the minute Algæ,—as the simplest examples of rhythmic movements—simplest in regard of method, of structure, of apparent uniformity of circumstances, and of spontaneity. And if I were to enumerate all the instances of rhythmic motion that I can find, other than those of hearts, their variety would suffice to show, that for the explanation of rhythm, we must find something much wider than any peculiarity in the structure or nerve-supply of hearts. The time-regulated movements of Oscillatoriæ and their congeners; those of the lateral leaflets in *Desmodium gyrans*, and of the labellum in *Megaclinium falcatum**; the movements of spermatozoa and of their analogues in the vegetable kingdom; the constant rhythm of the nutritive yelk in the ovum of the Pike†; the movements of the pulmograde Acalephæ; the rhythmic actions of the larger veins in the Bat's wing:—all these make a heterogeneous list if we look to structure or to office; their only apparent mark of resemblance is that they are all rhythmical in action, *i. e.* they all observe a rule of time in the manner of their action, a rule in which there is a regulation not only of the times at which successive and alternate actions follow one another, but of those during which each action is continuous.

But there is another thing common to all rhythmically acting organs: they are all the seats of nutritive processes; and I believe that their movements are rhythmical, because their nutrition is so; and rhythmic nutrition is, I believe, only a peculiar instance, or method of manifestation, of a general law of Time as concerned in all organic processes. In other words, I believe that rhythmic motion is an issue of rhythmic nutrition, *i. e.* of a method of nutrition, in which the acting parts are at certain periods raised, with time-regulated progress, to a state of instability of composition, from which they then decline, and in their decline may change their shape, and move with a definite velocity, or (as nervous centres) may discharge nerve-force.

Regarded as phenomena of nutrition, the chief things in which

* Lindley; and Morren in the 'Annales des Sciences Naturelles.'

† Reichert in Müller's 'Archiv,' 1857, p. 46.

rhythmic changes, whether attended with motion or not, may seem strange, are (1) that they are not continuous, but interrupted, and, as it were, alternating between action and inaction, or between progress and regress; and (2) that they are very minutely observant of time. But, in both these regards, they are examples of very general laws of organic processes; alternations of action and of rest, or of opposite actions, being common phenomena of organic life, and all organic processes being regulated with exact observance of time.

In all organic processes, laws of time are observed as exactly as are those concerning weight, and size, and composition.

In the largest view, in that cycle of mutations, according to a parental type, in which the life of every individual (at least among the higher organisms) consists, the successive changes are accomplished with the same exact regard to time as to every other particular. The offspring at each period of its course attains the same condition as the parent at the corresponding period had attained; the rules of weight, of shape, of composition, and of time, are all alike observed in the reproduction of the parent in the offspring.

And, within this view, we may observe how all the parts, or even all the elemental structures, of any organism, keep time in its development. Prematurity is, probably, even more rare than is preponderance, among them.

And so to natural death; for rare as it may be, there is a death even among men, in which, with uniform and synchronous decay, all parts arrive at the same time at the stage of incapacity for work.

It is the same everywhere. How evident is the observance of a law of time in the organic phenomena of the seasons! or, with more minute regulation, in those of sleep and waking, not only among animals, but, much more exactly, in the leaves and flowers of plants; or in their unfoldings; or in the movements of stamina, the dehiscence of fruits and the like, whether in succession or coincidently! In all of these, occurring as they do precisely when they are necessary for the welfare of the individual or the race, we may observe a time-regulation of the organic processes, which, for its precision, passes calculation. For though many of these processes may be very dependent, as to the variations of their rate, upon external conditions, and especially upon variations of light and heat, yet their mean or proper rate is not explained by these conditions, nor is wholly

due to them; a distinct law of periodicity, various according to the species, is observed in all; and even what the external conditions do effect, they effect by their influence on processes of cell-life, which they can alter in respect of time in only the same measure, and with the same limitations, as they can alter them in respect of quantity, or any other character.

Instances of the time-regulation of processes in the animal economy are as evident: witness the returns of thirst and hunger, the regulated times for digestion, the rates of excretions, the daily risings and fallings of temperature, the times for the development of ova, for the growth and development of the uterus, and of all the parts appertaining to gestation and to lactation.

Equally is time observed in processes of disease. It is most evident, in one sense, in all intermittent and other periodic affections; but, in another way, as evident in eruptive fevers. In small-pox, for example, not only is the time set in which the complicated process of eruption will be achieved, but, though every pustule is, in its appearance, independent of the rest, yet all are together in regard of time, because there is the same time-regulation for all. And so, when we watch the progress and the natural remedy of some spreading disease, such as erysipelas or gangrene, the observance of time is, in the whole process, as exact as that of quantity, or any other characteristic of the disease.

I dwell the more on these facts, because, familiar as they are to all, their importance as indicating a general law of organic life seems to be overlooked, and especially their bearing on the explanation of the rhythmic action of the heart has not been duly considered. For not only are many of the processes to which I have referred instances of regular and time-ordered alternations of action, and therefore, in the strict sense, rhythmical; but they are all instances of that exact observance of minute periods of time, which, on a superficial view, appears as the most singular and inexplicable character of the rhythm of a heart. For, in all these cases, the final correctness of the result of the organic process, the punctuality of its event, is proof that it was exactly timed in every stage; that it was, throughout, regulated to the second; just as the action of the heart is, or as the accuracy of a chronometer, at a year's end, is proof, or very nearly proof, that it kept right time in every hour and every second

of the year. In the maturation or the development of an ovum, for example, or in the ripening of seeds, a law of time, various in different species, is observed by all, and their punctuality in arriving at the climax of maturity, after many hours or days or months of progressive change, is evidence that they were chronometric in all their course thitherward. And where many such processes are concurrent, though severally independent, as in the contemporary maturation of all the ovules of an ovary, we may say that their concurrence is like that of many exact chronometers; and their final punctuality, like that of the chronometers, would prove that, in each unit of time, each did a certain and proportionate amount of work.

In all organic processes, then, there is as minute a regulation of time as there is of quantity, or shape, or quality of matter. Time-work is not a singular characteristic of quickly rhythmic organs; it is a rule of life; and its rate in each organism is neither determined, nor beyond certain limits alterable, by external conditions, or by any appreciable qualities of weight or composition (as are the time-relations of inorganic masses); but is determined by properties inherited, and inherent in the very nature of the organism, and is least alterable by external conditions in the highest organisms.

But though the general law of chronometric nutrition (if I may so call it) may be evident, yet it may be objected by some, that it is proved only for such nutritive processes as are long-continuous and cumulative; and that it is an unwarranted assumption to think of a rhythmical or frequently interrupted nutrition. To which objection the answer may be, that whether we regard a rhythmic nutrition as the cause of rhythmic motion or not, we are obliged to hold such a method of nutrition as a fact. For we can be nearly certain that in the heart, as in other muscular, or any other parts, the successive impairments and renovations of composition, which constitute the process of nutritive maintenance, are severally accomplished during the successive periods of action and of repose, all exercise being attended with impairment of composition, such as can be repaired only during repose. Now the only repose of the heart's muscles, and I suppose of its nervous system also, is in the brief intervals between their successive actions; and in these intervals, and, therefore, with a rhythmic nutrition coordinate with its rhythmical action, the heart-structures must recover from the changes suffered

in their actions. Whether, then, as a cause, or as a consequence, where there is rhythmical action there must be corresponding rhythmical waste and repair; for we cannot reasonably suppose that the heart, or any other similarly acting organ, has, as a special prerogative, an exemption from the law of impairment in or by exercise: such an exemption is, indeed, inconceivable.

Now if rhythmic nutrition be thus proved as a necessary attendant of rhythmic action, it must be regarded as the cause, not the consequence, of the action; for in all cases nutrition has precedence of other actions in organized bodies; and the time-regulation of nutrition is a general and principal fact, and is a cause, not a consequence, of many phenomena which we trace in other organs than the heart, and many of which are attended with time-ordered movements.

I suspect that, to many, that which will seem most difficult of acceptance, is the belief that in so quick rhythmic actions as that of the mammalian heart (for example), or that of cilia, there can be a corresponding quickness of alternation of the progressive and retrogressive changes which essentially constitute nutrition. It must be admitted that, when we watch these movements, they appear, at first sight, very unlike anything that can result from nutritive changes, in which we are apt to think of a certain deliberation and quietness. But all rhythmic movements are not thus rapid; and when we watch the actions of a heart reduced to move only once in two or three minutes (as a frog's may be by ligature around the venous sinus), the appearance is like nothing more than it is like that of a process of nutritive changes, in which the structures gradually reach a climax of instability, and then quickly change. Whatever value then there may be in the appearance of a rhythmic action as an indication of its cause, it might be adduced on either side of the inquiry.

But let me add, that the nutritive changes to which I here refer, do not involve the supposition of any rapidly successive making and unmaking of the structures of the rhythmic organ, whether the heart or any other. We have probably held too much of the making and unmaking of elemental parts as essential to their maintenance by nutrition. In the modelling of parts during development and growth, such complete changes probably occur; but in mere maintenance of parts there is no evidence of their frequent or ordinary

occurrence, and to assume it is contrary to the fact, that we rarely find any rudimental structures among the perfect ones. In the most active muscles of the adult, for example, I doubt whether a rudimental or developing fibre could be found; we have sufficient chemical evidence of a constant change of material in them, but no evidence of an equal or parallel change of structure. And so in the blood: the change of material is very rapid, but the change of structures, which we may in some measure estimate by the proportion of white or rudimental blood-cells, is probably slow*. And again, in the secreting glands, excepting those of the skin and the breasts, we have no evidence, and, I think, no sufficient reason to believe, that in all cases the gland-cell-walls dissolve or burst in the act of secretion, so as to need the entire new formation of fresh cells. For we find in most of the active glands no considerable number of either rudimental or degenerate cells; and the observations of Ludwig and Rahn on the secretion of saliva indicate, as many other facts do, that in ordinary secretion (which is the ordinary nutrition of glands) the cell-contents, gradually transformed, flow out through the persistent cell-walls.

Nutritive maintenance, then, probably requires nothing more than molecular substitution. Atoms even of the refuse substance may be passing out, and atoms of the renewing substance passing into places among the structures of a comparatively persistent framework. Cell-walls or their analogues may be long-lived, while their contents are undergoing continual mutation. Such a process of molecular interchange and passage is, indeed, visible in the absorption of oil through the epithelial cells and villi of the intestines; and this is probably only a coarse example of the ordinary manner in which cells change their contents in the nutritive processes. Changes like these may well consist with the quickest rhythmic action.

I would thus, then, conclude as to the most probable explanation of the rhythmic action of the heart:—

1. In the Vertebrata it is due to the time-regulated discharges of

* The very small quantity of iron, in proportion to the quantities of the other constituents of the blood-cells, found in the excretions, is another indication of the comparatively slow waste of the red blood-cells, and may suggest, besides, that in nutritive maintenance there is not an equal mutation of all the component substances of a structure.

nerve-force in certain of the ganglia in and near the substance of the heart, by which discharges the muscular walls are excited to contraction.

2. In Invertebrata, the corresponding pulsatile movements of hearts or vessels are probably independent of nerve-force.

3. The time-regulated rhythmic action, whether of the nervous centres or of the independent contractile walls, is due to their nutrition being rhythmic, *i. e.* to their being, in certain periods, by nutritive changes of composition, raised, with regulated progress, to a state of instability of composition, in their decline from which they discharge nerve-force, or change their shape, contracting.

4. The muscular substance of the heart in the Vertebrata, governed in its rhythmic action by appropriate nervous centres, has a rhythmic nutrition of its own, corresponding and coordinate with theirs; the impairments of its structure during action being repaired in repose.

5. Rhythmic nutrition is a process in accordance with the general laws of organic life, very many organic processes being composed of timely-regulated alternate action and inaction, or alternate opposite actions, *i. e.* being rhythmical, with larger or shorter units of time; and all organic processes being chronometric, *i. e.* ordered according to laws of time as exact, and only as much influenced by external conditions, as are those relating to weight, size, shape, and composition.

June 15, 1857.

The LORD WROTTESLEY, President, in the Chair.

The following gentlemen were admitted into the Society:—

The Rev. T. Romney Robinson, D.D.
Lionel Smith Beale, Esq.
George Grote, Esq.
Rowland Hill, Esq.
The Rev. Thomas Kirkman.
William Marcet, M.D.
John Marshall, M.D.
Andrew Smith, M.D.
John Welsh, Esq.

The following communications were read:—

I. "On a new Mode of forming Triethylamine." By A. W. Hofmann, LL.D., F.R.S. Received May 23, 1857.

Researches on the constitution of the nitrogenous organic bases, which I zealously prosecuted some years ago, and the result of which were communicated to the Royal Society, have been, by circumstances, interrupted for some time. Nearly all the observations recorded in my communications refer to the primary or amidogen-bases, *i.e.* the compound ammonias, in which 1 equivalent of hydrogen is replaced by an organic molecule. Of the secondary and tertiary bases (imidogen- and nitrile-bases), *i. e.* the ammonias in which 2 or 3 equivalents of hydrogen are replaced, and of the fixed ammonium-bases, little more is known at present than their mode of formation and their composition.

On again taking up this subject lately, it appeared of paramount importance to search for new and, if possible, simpler methods of forming the secondary and tertiary bases.

Of the several experiments made for this purpose, on which I intend to report at another opportunity, I may be allowed even now to quote a result which appears to be capable of a more general application.

One of the common methods of preparing the amidogen-bases, consists in treating cyanic ether with hydrate of potassa, a reaction which was first observed by Wurtz. To take a special case, cyanate of ethyl, when treated with hydrate of potassa, assimilates 2 equivalents of water and splits into ethylamine, which is set free, and carbonic acid, which is fixed by the alkali. An appropriate modification of this reaction appeared to afford a means of passing directly from cyanate of ethyl to *triethylamine*. For this purpose it was only necessary to offer to the cyanate of ethyl 2 equivalents of oxide of ethyl instead of 2 equivalents of water. The action of cyanate of ethyl upon ethylic potassa appeared to involve the conditions necessary for accomplishing this result.

Experiment has fully confirmed this anticipation.

On digesting for several hours at a moderate temperature a mix-

ture of pure cyanate of ethyl with pure ethylic potassa or soda (that is, absolute alcohol as far as possible, saturated with potassium or sodium), and distilling subsequently upon a sand-bath, a strongly alkaline liquid containing alcohol is obtained. Saturated with hydrochloric acid, this liquid furnishes on evaporation to dryness a residue from which a considerable quantity of *triethylamine* is expelled on addition of potassa.

The analogy of the two processes, of the formation of ethylamine and triethylamine, is obvious on glancing at the following equations.

$$\underset{\text{Cyanate of ethyl.}}{C_2ENO_2} + \underset{\text{Hydrate of potassa.}}{2(KO, HO)} = \underset{\text{Ethylamine.}}{N\left\{\begin{array}{l}E\\H\\H\end{array}\right.} + \underset{\text{Carbonate of potassium.}}{2KCO_3}$$

$$\underset{\text{Cyanate of ethyl.}}{C_2ENO_2} + \underset{\text{Ethylic potassa.}}{2(KO, EO)} = \underset{\text{Triethyl-amine.}}{N\left\{\begin{array}{l}E\\E\\E\end{array}\right.} + \underset{\text{Carbonate of potassium.}}{2KCO_3}$$

There could be no doubt regarding the identity of the basic compounds thus formed with triethylamine. The base floating on the surface of the saturated aqueous solution, possessed in a marked manner the characteristic odour of triethylamine. In order to exclude the possibility of a mistake, the hydrochloric solution of the compound was mixed with a solution of bichloride of platinum. Only after protracted standing of the highly concentrated solution, deep orange-coloured, well-formed crystals of the platinum-salt were deposited, the physical characters of which were still fresh in my memory.

The determination of platinum furnished results closely agreeing with the formula

$$N\left\{\begin{array}{l}E\\E\\E\end{array}\right. HCl,\ Pt\,Cl_2$$

I have already applied this reaction in various directions, and shall communicate the results which I have obtained at a future period. On considering how frequently, in the action of hydrated potassa upon organic substances, the hydrogen of the water of

hydration is assimilated by the products of decomposition, the substitution of ethylic potassa for the hydrate appears in many cases to promise valuable results from this mode of ethylation. The facility, however, with which ethylic potassa decomposes at a comparatively low temperature, must always be a serious obstacle to an extensive use of this reaction.

II. "Contribution towards the History of Thialdine." By A. W. Hofmann, LL.D., F.R.S. Received May 30, 1857.

Researches into the behaviour of several organic bases with bisulphide of carbon, which I intend to lay before the Society at a later period, have led to some experiments with thialdine, the results of which I may be allowed briefly to state, since they are but loosely connected with the principal object of the inquiry.

It appeared of interest to gain some insight into the constitution of thialdine, which is the prototype of an important class of alkaloids containing sulphur. To what group of bases does this body belong? Is it a primary, a secondary, or a tertiary base? The favourite method of questioning bases by means of iodide of methyl promised to furnish some elucidation of this subject.

Thialdine, dissolved in iodide of methyl, to which conveniently its own bulk of ether has been added, furnishes after twelve hours a solid crystalline mass, which may be readily freed from adhering thialdine by washing with ether and recrystallizing from alcohol.

The crystalline compound produced in this manner is the iodide of a methylated base, generated, as proved by analysis, by the union of equal equivalents of the two constituents.

$$\underbrace{C_{12}H_{13}NS_4}_{\text{Thialdine.}} + \underbrace{C_2H_3I}_{\substack{\text{Iodide of} \\ \text{methyl.}}} = \underbrace{C_{14}H_{16}NS_4I}_{\substack{\text{Iodide of} \\ \text{methylthialdine.}}}$$

The deportment of the new iodide resembles that of iodide of tetramethylammonium, and the analogous iodides, which I have described some years ago. Insoluble in ether, soluble in alcohol, and precipitated from this solution by ether in the crystalline state, soluble in water with an acid reaction, and separated from it again,

unchanged by potassa in the cold, this substance exhibits in general all the characters which distinguish the iodides of the so-called ammonium-bases.

The great mobility of the elements in thialdine, and especially the large amount of sulphur which it contains, afforded but little hope of successfully submitting the new iodide to the experiment with oxide of silver, which is so characteristic for the ammonium-bases. On adding oxide of silver to the aqueous solution of this body, which, as has been remarked already, possesses an acid reaction, iodide of silver is formed, and the liquid assumes at once a marked alkaline reaction; but since the almost simultaneous formation of sulphide of silver, and a powerful evolution of aldehyde, sufficiently indicate the perfect destruction of the compound, and since experiment denotes the presence of ammonia in the liquid, it is impossible to decide whether the alkaline reaction observed after decomposition is due to the liberation of an ephemeral ammonium-base, or to its product of decomposition.

My experiments appear nevertheless to establish that thialdine belongs to the tertiary bases,—that it is a nitrile-base.

$$\text{Ammonia } N\begin{cases} H \\ H \\ H \end{cases}$$

$$\text{Thialdine } N\{ C_{12}\, H_{13}\, S_4.$$

To the complex molecule, $C_{12}\, H_{13}\, S_4$, we must ascribe the faculty of replacing the 3 equivalents of hydrogen in the ammonia. In what manner, however, the elements are grouped in this complex molecule, whether we have here to assume a very unstable tribasic radical, or whether special molecules are inserted for each of the hydrogen-equivalents in the ammonia;—to decide these points further experiments are required.

Liebig and Wöhler, in their memoir on thialdine, state that the whole of the nitrogen of this base is eliminated in the form of ammonia, if the compound be decomposed by nitrate of silver. This deportment appeared to furnish an easy mode of controlling the formula of methylthialdine; after the decomposition of this body by nitrate of silver, I expected to find the whole of the nitrogen in the residuary liquid in the form of methylamine. On performing

the experiment, it was found that the liquid contained considerable quantities of ammonia, but together with the latter alkali a fixed base. This unexpected deportment reminded me of experiments published several years ago *, on the action of oxide of silver on thialdine, which is said to be thus converted into *Leucine*. The question presented itself, were the phenomena observed in the present case of an analogous character? had the above experiment given rise to the formation of methylated *Leucine*?

Experiment proved that the decomposition of iodide of methylthialdine by oxide of silver gives rise to the formation of ammonia and of the oxide of tetramethylammonium

$$\left.\begin{matrix} C_2\,H_3 \\ C_2\,H_3 \\ C_2\,H_3 \\ C_2\,H_3 \end{matrix}\right\} NO, HO,$$

which was satisfactorily identified by the analysis of the gold- and platinum-compound.

The following equation represents this change:—

$$\underbrace{4C_{14}\,H_{16}\,NS_4\,I}_{\text{Iodide of methyl-thialdine.}} + 20AgO + 2HO = 4AgI + 16AgS + \underbrace{12C_4\,H_4\,O_2}_{\text{Aldehyde.}}$$
$$+ 3H_3\,N + \underbrace{C_8\,H_{12}\,NO,HO}_{\text{Hydrate of the oxide of tetramethylammonium.}}$$

The absence of any leucine-like compound among the products of decomposition of iodide of methylthialdine induced me to repeat the experiment on the action of oxide of silver upon thialdine itself.

The transformation of thialdine into leucine, announced several years ago, and apparently very intelligible by the analogy of the formulæ of the two bodies

Thialdine .... $C_{12}\,H_{13}\,NS_4$
Leucine...... $C_{12}\,H_{13}\,NO_4$,

has not failed to rivet the attention of chemists. I wonder how such an error could have crept into science; for I have convinced myself, by a series of careful experiments, that this assertion is without the slightest foundation.

Thialdine, when treated with oxide of silver, as might have been

* Goessmann, Liebig's Annalen, xc. 184.

expected, undergoes the same decomposition which Liebig and Wöhler had effected by means of nitrate of silver. It furnishes in this process aldehyde, acetic acid, and ammonia, but *no leucine.*

In conclusion, a remark which refers to a question connected with this alleged formation of leucine:—Soon after the statement regarding this subject had been made, Wagner* observed that the action of bisulphide of carbon upon amylamine gives rise to the formation of a crystalline compound. This compound was not analysed, but Wagner suggested that possibly it might be thialdine,

$$\underset{\text{Amylamine}}{C_{10}H_{13}N} + 2CS_2 = \underset{\text{Thialdine}}{C_{12}H_{13}NS_4},$$

and that in case this assumption proved to be correct, the action of bisulphide of carbon upon the homologues of ammonia might lead to the artificial formation, not only of leucine, but of glycocine, alanine, &c.

During my experiments on the deportment of bisulphide of carbon with organic bases, I have also had occasion to study the compound of amylamine with bisulphide of carbon. It is only necessary superficially to compare this substance with thialdine, in order to perceive at once that they are two absolutely different compounds; and if there is still a chance of producing leucine from amylamine, it is because the action of bisulphide of carbon does *not* give rise to the formation of thialdine.

III. "On the Action of Sulphuric Acid upon Anisic and Salicylic Acids." By A. W. Hofmann, LL.D., F.R.S. Received June 5, 1857.

In a paper read before the Royal Society, we have, Mr. Buckton and myself, directed attention to the general occurrence of the disulpho-acids, of which only few and scattered examples were known at that time.

M. Louis Zervas has since continued the study of this subject in my laboratory, and has arrived at the following results.

* Journal für practische Chemie, lxi. 505.

Anisic acid, treated with Nordhausen sulphuric acid, at a temperature of 100°, assimilates the elements of 2 equivs. of sulphuric acid, and is converted into a crystalline acid, containing

$$C_{16}\,H_8\,O_6 + 2HSO_4 = C_{16}\,H_8\,S_2\,O_{12} + 2aq.$$

The formula of this acid, which M. Zervas calls *sulphanisic acid*, was controlled by the analysis of a crystalline lead-salt and a barium compound, which were found to contain respectively.

Lead-salt .... $C_{16}\,(H_6\,Pb_2)\,O_{12} + 2aq.$
Barium-salt .. $C_{16}\,(H_6\,Ba_2)\,O_{12} + 2aq.$

If the mixture of anisic and fuming sulphuric acid be submitted to higher temperatures than that of boiling-water, a lively evolution of carbonic acid takes place, containing, if the temperature be maintained within the proper limits, scarcely a trace of sulphurous acid. After several hours' digestion, the solution contains no longer anisic or sulphanisic acid, but a new acid, which has the composition

$$C_{14}\,H_8\,S_4\,O_{14}.$$

The formula of this acid, which could not be obtained in a state fit for analysis, was fixed by the analysis of a barium-salt, which was found to contain

$$C_{14}\,(H_6\,Ba_2)\,S_4\,O_{14} + 2aq.$$

This acid may be considered as formed by the association of 1 equivalent of anisol (the product of the action of alkaline earths upon anisic acid at a high temperature) with 4 equivalents of anhydrous sulphuric acid. M. Zervas has, in fact, experimentally proved, that his acid may be obtained directly by treating anisol with fuming sulphuric acid, and he accordingly proposes to designate this compound by the name of *disulphanisolic acid.*

From the preceding experiments it is obvious that the action of sulphuric acid upon organic acids with 6 equivalents of oxygen, is perfectly similar to that which this agent exerts upon acids with 4 equivalents of oxygen.

Acetic acid.

1st Stage .. $C_4\,H_4\,O_4 + 2HSO_4 = C_4\,H_4\,S_2\,O_{10} + 2HO.$
2nd Stage.. $C_4\,H_4\,O_4 + 4HSO_4 = C_2\,H_4\,S_4\,O_{12} + 4HO + 2CO_2.$

Anisic acid.

1st Stage $C_{16}H_8O_6+2HSO_4=C_{16}H_8S_2O_{12}+2HO$.

2nd Stage $C_{16}H_8O_6+4HSO_4=C_{14}H_8S_4O_{14}+4HO+2CO_2$.

There can be no doubt that salicylic acid, so closely allied to anisic acid, in fact, its homologue, a step lower in the system, must exhibit a similar deportment. Mr. Baldwin Duppa has been occupied with this subject in my laboratory, and has already obtained the first term, the *sulpho-salicylic acid,*

$$C_{14}H_6S_2O_{12},$$

which forms extremely beautiful compounds.

On submitting salicylic acid to the action of an excess of sulphuric acid, at a temperature of 180°, the same phenomena present themselves which are noticed in the case of salicylic acid, a powerful evolution of carbonic acid ensues, and the solution now contains a new sulphur-acid, which is crystalline. As yet Mr. Duppa has not succeeded in obtaining this acid in a state of sufficient purity for analysis, but it may be assumed, without much hesitation, that it will be found to be

Disulphophenic acid.... $C_{12}H_6S_4O_{14}$.

IV. "On the Separation of Iodine, Bromine, and Chlorine, and the comparative degree of Affinity of these Elements for Silver; with some Analyses of their Combinations with that Metal occurring in Chili." By Frederic Field, Esq. Communicated by Dr. Hofmann, F.R.S. Received June 5, 1857.

Although both bromide and iodide of silver are decomposed by the action of chlorine at an elevated temperature, yet chloride of silver is completely decomposed by bromide of potassium, and both the bromide and chloride of silver by iodide of potassium. Even the action of hot strong hydrochloric acid has but little influence upon the iodide of silver; many days of continuous boiling are necessary for its entire decomposition. I believe that it has

been the opinion of chemists that chlorine possesses an affinity for silver superior to all other elementary bodies, and we are told in Gmelin's Handbook that all salts of silver, even the insoluble ones, are converted into chloride by solutions of metallic chlorides. From the following experiments it appears to me that bromine has a greater affinity for silver than chlorine, and iodine a still greater affinity than bromine.

When a mixed solution of bromide of potassium and chloride of sodium is added gradually to a solution of nitrate of silver, not in excess, no trace of chloride of silver is precipitated, as long as any bromide remains in solution.

If to a similar solution, iodide and bromide of potassium and chloride of sodium be added, iodide of silver and nitrate of potassa are formed, the bromide of potassium and chloride of sodium remaining undecomposed.

When bromide of potassium is poured upon chloride of silver, an entire decomposition ensues, bromide of silver and chloride of potassium being produced.

When iodide of potassium is added to chloride of silver, iodide of silver and chloride of potassium are formed; and when iodide of potassium is added to bromide of silver, there is a similar decomposition, the iodine replacing the bromine.

When chloride of silver in excess is agitated with a solution of iodide of potassium and warmed for some hours, no trace of iodine can be detected in the solution: when however chloride of sodium is poured upon iodide of silver, no decomposition occurs, neither is there any action upon bromide of silver with the same salt: and when bromide of potassium is added to iodide of silver, there is no alteration in the union of the elements.

From a number of experiments made in illustration of the preceding statements, I deemed it possible that the separation of chlorine, bromine and iodine could be accomplished by this reaction.

The method which I have devised is simply this: After weighing three equal portions of the salts to be analysed, they are placed in three flasks with ground-glass stoppers, and about an ounce of water is added to each; nitrate of silver being then added, slightly in excess, to the three, the stoppers are replaced, and each flask agitated violently. The precipitates subside in a few minutes, leaving the

supernatant liquid perfectly clear. They are then filtered through separate funnels, and washed with hot water. No. 1 is dried and weighed. No. 2 is digested in bromide of potassium, dried and weighed; and No. 3 in iodide of potassium, dried and weighed.

To test the method, a mixture was made of 5 grains of iodide of potassium, 5 grains of bromide of potassium, and 5 grains of chloride of sodium. The following is a comparison of the theoretical and experimental results:—

	EXPERIMENT.	THEORY.
Iodine	3·69	3·81
Bromine	3·51	3·34
Chlorine	2·92	3·02

I have availed myself of this method in analysing several silver ores containing chloride, bromide and iodide of silver found in Chili, the formulæ of which I subjoin:—

Chloride of silver	Ag Cl.
Chlorobromide of silver	2Ag Cl, Ag Br.
Chlorobromide of silver	3Ag Cl, 2Ag Br.
Chlorobromide of silver	Ag Cl, 3Ag Br.
Bromide of silver	Ag Br.
Iodide of silver	Ag I.

V. "Note on the Density of Ozone." By Thomas Andrews, M.D., F.R.S., Vice-President of Queen's College, Belfast, and P. G. Tait, M.A., Fellow of St. Peter's College, Cambridge, and Professor of Mathematics in Queen's College, Belfast. Received June 17, 1857.

It is known that Ozone can only be obtained mixed with a large excess of oxygen. In a former communication by one of the authors of this note, it was shown that in the electrolysis of a mixture of 8 parts of water and 1 of sulphuric acid, the mean quantity of Ozone does not exceed ·0041 gramme in a litre of oxygen, or $\frac{1}{350}$th part. By using a mixture of equal volumes of acid and water, the relative quantity of Ozone may be doubled; but even with the Ozone in this

more concentrated state, the ordinary methods of determining the density of a gas are plainly inapplicable. The difficulty of the problem was farther increased by the rapid action of Ozone on mercury, which rendered it impossible to collect or measure the gas over that metal; and the tension of aqueous vapour, as well as the gradual destruction of Ozone by water, prevented the use of the latter. After numerous trials, the method finally employed was to measure the change of volume which occurs in exposing a gaseous mixture containing Ozone to a temperature of 230° C., or upwards. The volume of the gas after this treatment was invariably found to have increased; and by eliminating the effects of alteration of temperature and pressure during the course of the experiment, by the aid of a similar vessel to that containing the Ozone, the authors succeeded in estimating the change of volume which took place, to an extremely small fraction of the entire amount.

The vessels employed in different experiments varied in capacity from 200 CC. to 600 CC., and terminated in tubes of about 2 millimetres in diameter, bent in a U-form and containing sulphuric acid. The amount of Ozone was ascertained by passing a stream of the gas through two other vessels, one placed on each side of the vessel to be heated, and afterwards analysing their contents by the method described in the communication already referred to. It was easy to measure with certainty a change of pressure amounting to $\frac{1}{60,000}$th of the whole; but on account of the ordinary fluctuations of atmospheric pressure between two consecutive observations of the primary and auxiliary vessels, it was rarely possible to work to this degree of accuracy.

The experimental data have not yet been completely reduced, and some slight corrections have yet to be investigated; but the general result of the inquiry, which has been a very protracted one, gives—on the assumption that Ozone is oxygen in an allotropic condition—for its density as compared with that of oxygen, nearly the ratio of 4 to 1.

The following approximate formulæ were employed in the reduction of the experiments. They are sufficiently exact for the purpose of calculation on account of the smallness of the quantities observed.

I. To reduce the change of level observed in the auxiliary vessel

during the interval of the experiment to the equivalent quantity for the primary vessel—

$$\left(\frac{2}{a}+\frac{1}{\mathrm{H}}\right)\delta x=\left(\frac{2}{a}+\frac{1}{\mathrm{H}_{\prime}}\right)\delta x_{\prime}.$$

II. To deduce from the corrected change of level in the primary vessel the relative density of Ozone and oxygen—

$$\frac{e-1}{e}=m\left(\frac{2}{a}+\frac{1}{\mathrm{H}}\right)\delta x_{\prime\prime}.$$

In these formulæ

a is the barometric pressure in terms of the sulphuric acid in the U-tubes.

H, the length of a tube of the same diameter as the U-tube of the primary vessel, and whose capacity is equal to that of the same vessel measured to the mean level of the acid in the U-tube.

$\mathrm{H}_{\prime}$, the same quantity for the auxiliary vessel.

$\delta x_{\prime}$, one-half of the change in the difference of levels in the U-tube of auxiliary vessel.

δx, the corresponding quantity for the primary vessel.

$\delta x_{\prime\prime}$, half the observed change in the primary vessel corrected by the quantity δx.

m, the ratio of the weights of oxygen and Ozone in the gaseous mixture.

e, the relative density of Ozone and oxygen.

VI. "Contributions towards the History of the Phosphorus-, Arsenic-, and Antimony-Bases." By A. W. Hofmann, Ph.D., LL.D., F.R.S. Received June 18, 1857.

The facility with which the phosphorus-bases can be obtained by the action of zincethyl upon terchloride of phosphorus, has induced me to study the action of this agent upon the terchlorides of arsenic and antimony. I have found that triethylarsine and triethylstibine, hitherto obtainable only with difficulty, by submitting the alloys of

potassium with arsenic and antimony to the action of iodide of ethyl, may be procured by this process as readily and as abundantly as the phosphorus-bases.

In attempting to identify the substances obtained by the new process with those prepared by the old one, by the analysis of platinum- and gold-salts, I have been led to the discovery of a series of compounds of remarkable beauty.

Since the study of these bodies, and especially of their derivatives, which are particularly numerous and interesting, will involve considerable time, I beg leave to submit to the Royal Society, before the session closes, a brief sketch of those substances, the composition of which I have already established by analysis.

On adding a solution of bichloride of platinum to an alcoholic solution of triethylphosphine, of triethylarsine or triethylstibine as long as the colour of the platinum solution entirely disappears, the liquid deposits after a few moments magnificent slightly yellow or colourless crystals. The three salts thus formed may be considered as the chlorides of three compound ammoniums containing platinum and ethyl, united respectively with phosphorus, arsenic and antimony. They have the following composition:—

$$\text{Chloride of Platophosphethylium} \ldots \left.\begin{matrix} Pt \\ E \\ E \\ E \end{matrix}\right\} P\,Cl.$$

$$\text{Chloride of Platarsethylium} \ldots\ldots \left.\begin{matrix} Pt \\ E \\ E \\ E \end{matrix}\right\} As\,Cl.$$

$$\text{Chloride of Platostibethylium} \ldots\ldots \left.\begin{matrix} Pt \\ E \\ E \\ E \end{matrix}\right\} Sb\,Cl.$$

On substituting for the bichloride of platinum a solution of terchloride of gold, perfectly similar phenomena present themselves. The gold solution is entirely decolorized, and the colourless liquids deposit three gold compounds, which, remarkably enough, are of a dazzling white colour and silvery lustre.

Analysis has proved these salts to correspond to the platinum-salts just mentioned, the platinum being replaced by an equivalent quantity of gold.

The formulæ of these substances are—

Chloride of Auro-phosphethylium .. $\left.\begin{matrix}\mathrm{Au}\\ \mathrm{E}\\ \mathrm{E}\\ \mathrm{E}\end{matrix}\right\}\mathrm{P\,Cl}.$

Chloride of Auro-arsethylium .... $\left.\begin{matrix}\mathrm{Au}\\ \mathrm{E}\\ \mathrm{E}\\ \mathrm{E}\end{matrix}\right\}\mathrm{As\,Cl}.$

Chloride of Auro-stibethylium...... $\left.\begin{matrix}\mathrm{Au}\\ \mathrm{E}\\ \mathrm{E}\\ \mathrm{E}\end{matrix}\right\}\mathrm{Sb\,Cl}.$

The complementary products formed in these reactions remain in the mother liquors of the several salts, from which they separate in the form of oily compounds which gradually solidify into crystalline masses. These I have not yet examined.

A glance at the above formulæ shows that the new compounds which form the subject of this note correspond to the chloride of platammonium,

$$\left.\begin{matrix}\mathrm{Pt}\\ \mathrm{H}\\ \mathrm{H}\\ \mathrm{H}\end{matrix}\right\}\mathrm{N\,Cl}.$$

It remains to be ascertained whether the chloride of plato-diammonium,

$$\left.\begin{matrix}\mathrm{H}\\ \mathrm{H}\\ \mathrm{H}\end{matrix}\right\}\mathrm{N}\ \left.\begin{matrix}\mathrm{Pt}\\ \mathrm{H}\\ \mathrm{H}\\ \mathrm{H}\end{matrix}\right\}\mathrm{N\,Cl},$$

is likewise represented in the phosphorus-, arsenic-, and antimony-series.

VII. "On a New Series of Compounds derived from Ammonia and its Analogues." By Edward Frankland, Ph.D., F.R.S. Received June 18, 1857.

Although zincethyl and its homologues are now well known to be capable of replacing electro-negative elements by ethyl, &c., yet it could scarcely have been anticipated, that substitutions of an almost opposite character would be effected by the same reagent; neverthe-

less I find that zincethyl is capable of removing one of the atoms of hydrogen in ammonia and its analogues, and of replacing it by zinc, thus forming a series of bodies which strongly remind us of the amide and nitride of potassium. The general nature of this reaction by which the compounds described below are produced, may be thus expressed:—

$$N\left\{\begin{matrix} x \\ y \\ H \end{matrix}\right. + \left.\begin{matrix} Zn \\ C_4H_5 \end{matrix}\right\} = N\left\{\begin{matrix} x \\ y \\ Zn \end{matrix}\right. + \left.\begin{matrix} C_4H_5 \\ H \end{matrix}\right\}.$$

When dry ammoniacal gas is passed through an ethereal solution of zincethyl, it is rapidly absorbed, and soon afterwards torrents of a combustible gas free from nitrogen begin to be evolved. After the current has been continued for more than an hour, the absorption altogether ceases, and the ethereal liquid deposits a copious white amorphous precipitate, which yielded on analysis numbers leading to the formula

$$N\left\{\begin{matrix} H \\ H \\ Zn. \end{matrix}\right.$$

The analysis of the combustible gas proved that 1 vol. consumed 3·43 vols. oxygen, and generated 1·95 vol. carbonic acid; numbers almost identical with those yielded by hydride of ethyl, with which the gas is also identical in specific gravity. These results prove that zincethyl acts upon ammoniacal gas in the following manner:—

$$N\left\{\begin{matrix} H \\ H \\ H \end{matrix}\right. + \left.\begin{matrix} Zn \\ C_4H_5 \end{matrix}\right\} = N\left\{\begin{matrix} H \\ H \\ Zn \end{matrix}\right. + \left.\begin{matrix} C_4H_5 \\ H \end{matrix}\right\}.$$

The solid product of the reaction, for which I propose the name *zincamide*, is a white amorphous body insoluble in ether, and instantly decomposed by water and alcohol, with evolution of great heat, and in such a manner as to regenerate ammonia. Thus with water the following equation expresses the reaction:—

$$N\left\{\begin{matrix} H \\ H \\ Zn \end{matrix}\right. + \left.\begin{matrix} H \\ H \end{matrix}\right\} O_2 = N\left\{\begin{matrix} H \\ H \\ H \end{matrix}\right. + \left.\begin{matrix} Zn \\ H \end{matrix}\right\} O_2.$$

Heated with iodide of ethyl in a sealed tube at 145° C., zincamide gives iodide of zinc and iodide of diethylammonium—

$$N\left\{\begin{matrix} H \\ H \\ Zn \end{matrix}\right. + 2\left.\begin{matrix} C_4H_5 \\ I \end{matrix}\right\} = N(C_4H_5)_2H_2I + ZnI.$$

Nitride of Zinc.

Zincamide can bear a temperature of 200° C. without decomposition, but at a low red heat it is decomposed into nitride of zinc and ammonia:—

$$3N\left\{\begin{matrix}H\\H\\Zn\end{matrix}\right.=2N\left\{\begin{matrix}H\\H\\H\end{matrix}\right.+N\left\{\begin{matrix}Zn\\Zn\\Zn.\end{matrix}\right.$$

Nitride of zinc is a grey pulverulent body, which is neither fused, decomposed, nor volatilized at a red heat out of contact with air. It is decomposed by water with great violence; in fact, if the nitride be merely moistened with water, it becomes red-hot. Several analyses prove that the formula of nitride of zinc is

$$N\left\{\begin{matrix}Zn\\Zn\\Zn.\end{matrix}\right.$$

The study of the products of its decomposition by water shows that the reaction may be thus expressed:—

$$N\left\{\begin{matrix}Zn\\Zn\\Zn\end{matrix}\right.+6HO=N\left\{\begin{matrix}H\\H\\H\end{matrix}\right.+3\left.\begin{matrix}Zn\\H\end{matrix}\right\}O_2.$$

Zincphenylimide.

$$N\left\{\begin{matrix}C_{12}H_5\\H\\Zn.\end{matrix}\right.$$

This body is produced by the action of zincethyl upon anhydrous aniline. The reaction is so violent that it requires to be moderated by diluting the zincethyl with ether. Heat is evolved, and a large quantity of a combustible gas is rapidly generated. At length the liquid becomes a white, semisolid, opake mass. Analysis proved the gas to be pure hydride of ethyl, whilst the solid body regenerated aniline in contact with water. The production of zincphenylimide may therefore be thus expressed:—

$$N\left\{\begin{matrix}C_{12}H_5\\H\\H\end{matrix}\right.+\left.\begin{matrix}Zn\\C_4H_5\end{matrix}\right\}=N\left\{\begin{matrix}C_{12}H_5\\H\\Zn\end{matrix}\right.+\left.\begin{matrix}C_4H_5\\H\end{matrix}\right\}.$$

Zincphenylimide is a white amorphous body very similar to zinc-

amide. Like the latter body, it is instantly decomposed by water, aniline being regenerated:—

$$N\left\{\begin{matrix}C_{12}H_5\\H\\Zn\end{matrix}\right. + \left.\begin{matrix}H\\H\end{matrix}\right\}O_2 = N\left\{\begin{matrix}C_{12}H_5\\H\\H\end{matrix}\right. + \left.\begin{matrix}Zn\\H\end{matrix}\right\}O_2.$$

Diethylzincamine.

$$N\left\{\begin{matrix}C_4H_5\\C_4H_5\\Zn.\end{matrix}\right.$$

This body is produced by the action of zincethyl upon diethylamine. The reaction requires to be aided by heat. Pure hydride of ethyl is evolved, and the following equation expresses the reaction:—

$$N\left\{\begin{matrix}C_4H_5\\C_4H_5\\H\end{matrix} + \begin{matrix}Zn\\C_4H_5\end{matrix}\right\} = N\left\{\begin{matrix}C_4H_5\\C_4H_5\\Zn\end{matrix} + \begin{matrix}C_4H_5\\H\end{matrix}\right\}.$$

Its reactions are quite similar to those of the bodies above described.

Zincoximide.

$$N\left\{\begin{matrix}C_2O_2\\H\\Zn.\end{matrix}\right.$$

Dry oxamide and pure zincethyl are without any action upon each other at ordinary temperatures, but the heat of a water-bath is sufficient to establish a violent reaction, torrents of pure hydride of ethyl are evolved, and zincoximide combined with zincethyl remains in the retort. The following equation expresses the reaction:—

$$N\left\{\begin{matrix}C_2O_2\\H\\H\end{matrix}\right. + 2(ZnC_4H_5) = N\left\{\begin{matrix}C_2O_2\\H\\Zn\end{matrix} + \begin{matrix}Zn\\C_4H_5\end{matrix}\right\} + \left.\begin{matrix}C_4H_5\\H\end{matrix}\right\}.$$

Zincacetimide.

$$N\left\{\begin{matrix}C_4H_3O_2\\H\\Zn.\end{matrix}\right.$$

Acetamide and zincethyl act upon each other very violently; a large quantity of gas is evolved, which analysis proves to be hydride of ethyl. Zincacetimide is contained in the residue as a white, amor-

phous, pulverulent solid, which is reconverted into acetamide by contact with water.

These reactions establish the fact, that by the action of zincethyl one of the atoms of hydrogen, in ammonia and analogous nitrogen compounds, combines with ethyl and becomes replaced by zinc. It will be interesting to extend this reaction to the nitryles, especially such as contain electro-negative radicals of the othyl family, since we may thus expect to obtain a new series of double radicals, amongst which the ketones will perhaps find a home. I hope soon to lay the results of this extension of the investigation before the Royal Society.

VIII. "On a Class of Dynamical Problems." By Arthur Cayley, Esq., F.R.S. Received June 18, 1857.

There are a class of dynamical problems which, so far as I am aware, have not been considered in a general manner. The problems referred to (which might be designated as continuous-impact problems) are those in which the system is continually taking into connexion with itself particles of infinitesimal mass (*i. e.* of a mass containing the increment of time dt as a factor), so as not itself to undergo any abrupt change of velocity, but to subject to abrupt changes of velocity the particles so taken into connexion. For instance, a problem of the sort arises when a portion of a heavy chain hangs over the edge of a table, the remainder of the chain being coiled or heaped up close to the edge of the table; the part hanging over constitutes the moving system, and in each element of time dt, the system takes into connexion with itself, and sets in motion with a finite velocity an infinitesimal length ds of the chain; in fact, if v be the velocity of the part which hangs over, then the length vdt is set in motion with the finite velocity v. The general equation of dynamics applied to the case in hand will be

$$\Sigma\left\{\left(\frac{d^2x}{dt^2}-\mathrm{X}\right)\delta x+\left(\frac{d^2y}{dt^2}-\mathrm{Y}\right)\delta y+\left(\frac{d^2z}{dt^2}-\mathrm{Z}\right)\delta z\right\}dm$$

$$+\Sigma\left(\Delta u\delta\xi+\Delta v\delta\eta+\Delta w\delta\zeta\,\frac{1}{dt}\,d\mu=0,\right.$$

where the first line requires no explanation, in the second line ξ, η, ζ

are the coordinates at the time t of the particle $d\mu$ which then comes into connexion with the system; Δu, Δv, Δw are the finite increments of velocity (or, if the particle is originally at rest, then the finite velocities) of the particle $d\mu$ the instant that it has come into connexion with the system; $\delta\xi$, $\delta\eta$, $\delta\zeta$ are the virtual velocities of the same particle $d\mu$ considered as having come into connexion with and forming part of the system. The summation extends to the several particles or to the system of particles $d\mu$ which come into connexion with the system at the time t; of course, if there is only a single particle $d\mu$, the summatory sign Σ is to be omitted. The values of Δu, Δv, Δw are $\frac{d\xi}{dt}-u$, $\frac{d\eta}{dt}-v$, $\frac{d\zeta}{dt}-w$, if by $\frac{d\xi}{dt}$, $\frac{d\eta}{dt}$, $\frac{d\zeta}{dt}$ we understand the velocities of $d\mu$ parallel to the axes, after it has come into connexion with the system; but it is to be observed, that considering ξ, η, ζ as the coordinates of the particle $d\mu$, which is continually coming into connexion with the system, then if the problem were solved and ξ, η, ζ given as functions of t (and, when there is more than one particle $d\mu$, of the constant parameters which determine the particular particle), $\frac{d\xi}{dt}$, &c., in the sense just explained, cannot be obtained by simple differentiation from such values of ξ, &c.: in fact, ξ, η, ζ so given as functions of t, belong at the time t to one particle, and at the time $t+dt$ to the next particle, but what is wanted is the increment in the interval dt of the coordinates ξ, η, ζ of one and the same particle.

Suppose as usual that x, y, z, and in like manner that ξ, η, ζ are functions of a certain number of independent variables θ, ϕ, &c., and of the constant parameters which determine the particular particle dm or $d\mu$, of which x, y, z, or ξ, η, ζ are the coordinates, parameters, that is, which vary from one particle to another, but which are constant during the motion for one and the same particle. The summations are in fact of the nature of definite integrations in regard to these constant parameters, which therefore disappear altogether from the final results. The first line,

$$\Sigma\left\{\left(\frac{d^2x}{dt^2}-\mathrm{X}\right)\delta x+\left(\frac{d^2y}{dt^2}-\mathrm{Y}\right)\delta y+\left(\frac{d^2z}{dt^2}-\mathrm{Z}\right)\delta z\right\}dm,$$

may be reduced in the usual manner to the form

$$\Theta\delta\theta+\Phi\delta\phi+\ldots.$$

where, writing as usual θ', ϕ', &c. for $\frac{d\theta}{dt}, \frac{d\phi}{dt}$, &c.,

we have
$$\Theta = \frac{d}{dt}\frac{d\mathrm{T}}{d\theta'} - \frac{d\mathrm{T}}{d\theta} + \frac{d\mathrm{V}}{d\theta},$$
$$\Phi = \frac{d}{dt}\frac{d\mathrm{T}}{d\phi'} - \frac{d\mathrm{T}}{d\phi} + \frac{d\mathrm{V}}{d\phi}, \text{ \&c.,}$$

(this supposes that $\mathrm{X}dx + \mathrm{Y}dy + \mathrm{Z}dz$ is an exact differential); only it is to be observed that in the problems in hand, the mass of the system is variable, or what is the same thing, the variables θ, ϕ, &c., are introduced into T and V through the limiting conditions of the summation or definite integration, besides entering directly into T and V in the ordinary manner. And in forming the differential coefficients $\frac{d}{dt}\frac{d\mathrm{T}}{d\theta'}, \frac{d\mathrm{T}}{d\theta}, \frac{d\mathrm{V}}{d\theta}$, &c., it is necessary to consider the variables θ, ϕ, &c., in so far as they enter through the limiting conditions *as exempt from differentiation*, so that the expressions just given for Θ, Φ, &c., are, in the case in hand, rather conventional representations than actual analytical values; this will be made clearer in the sequel by the consideration of the before-mentioned particular problem.

Considering next the second line, or

$$\Sigma\left\{\left(\frac{d\xi}{dt} - u\right)\delta\xi + \left(\frac{d\eta}{dt} - v\right)\delta\eta + \left(\frac{d\zeta}{dt} - w\right)\delta\zeta\right\}\frac{1}{dt}d\mu,$$

we have here

$$\delta\xi = a\ \delta\theta + b\ \delta\phi + \ldots$$
$$\delta\eta = a'\ \delta\theta + b'\delta\phi + \ldots$$
$$\delta\zeta = a''\delta\theta + b'\delta\phi + \ldots,$$

where a, b, a', &c., are functions of the variables θ, ϕ, &c., and of the constant parameters which determine the particular particle $d\mu$. The virtual velocities or increments $\delta\theta$, $\delta\phi$, &c., are absolutely arbitrary, and if we replace them by $d\theta$, $d\phi$, &c., the actual increments of θ, ϕ, &c., in the interval dt during the motion, then $\delta\xi$, $\delta\eta$, $\delta\zeta$ will become $\frac{d\xi}{dt}dt$, $\frac{d\eta}{dt}dt$, $\frac{d\zeta}{dt}dt$, in the sense before attributed to $\frac{d\xi}{dt}$, $\frac{d\eta}{dt}, \frac{d\zeta}{dt}$.

The particle $d\mu$ will contain dt as a factor, and the other factor will contain the differentials, or as the case may be, products of differentials of the constant parameters which determine the particular

particle $d\mu$. We have thus the means of expressing the second line in the proper form; and if we write

$$\Sigma\ (a^2+a'^2+a''^2)\ d\mu=\mathrm{A}dt$$
$$\Sigma\ (b^2+b'^2+b''^2)\ d\mu=\mathrm{B}dt$$
$$\vdots$$
$$\Sigma\ (ab+a'b'+a''b'')\ d\mu=\mathrm{H}dt$$

$$\Sigma\ (au+a'v+a''w)\ d\mu=-\mathrm{P}dt.$$
$$\Sigma\ (bu+b'v+b''w)\ d\mu=-\mathrm{Q}dt.$$
$$\vdots$$

then the required expression of the second line will be

$$(\mathrm{A}\theta'+\mathrm{H}\phi'\ldots+\mathrm{P})\ \delta\theta+(\mathrm{H}\theta'+\mathrm{B}\phi'\ldots+\mathrm{Q})\delta\phi+\ldots$$

which, if we put

$$\mathrm{K}=\tfrac{1}{2}\ (\mathrm{A}\theta'^2+\mathrm{B}\phi'^2+\ldots+2\mathrm{H}\theta'\phi'+\ldots+2\mathrm{P}\theta'+2\mathrm{Q}\phi'+\ldots),$$
$$=\tfrac{1}{2}\ (\mathrm{A},\ \mathrm{B},\ \ldots\mathrm{H},\ \ldots\mathrm{P},\ \mathrm{Q},\ \ldots \between \theta',\ \phi',\ \ldots,\ 1)^2,$$

may be more simply represented by

$$\frac{d\mathrm{K}}{d\theta'}\ \delta\theta+\frac{d\mathrm{K}}{d\phi'}\ \delta\phi+\ldots$$

Only it is to be remarked that A, B, .. H, .. P, Q, .. will in general contain not only θ, ϕ, .., but also the differential coefficients θ', ϕ', .., and that in forming the differential coefficients $\frac{d\mathrm{K}}{d\theta'}$, $\frac{d\mathrm{K}}{d\phi'}$, &c., the quantities θ', ϕ', .., in so far as they enter into K, not explicitly, but through the coefficients A, &c., must be considered *as exempt from differentiation,* so that the preceding expression for the second line by means of the function K is rather a conventional representation than an actual analytical value.

Uniting the two lines, and equating to zero, the coefficients of $\delta\theta$, $\delta\phi$, &c., we obtain finally the equations of motion in the form

$$\frac{d}{dt}\frac{d\mathrm{T}}{d\theta'}-\frac{d\mathrm{T}}{d\theta}+\frac{d\mathrm{V}}{d\theta}+\frac{d\mathrm{K}}{d\theta'}=0,$$

$$\frac{d}{dt}\frac{d\mathrm{T}}{d\phi'}-\frac{d\mathrm{T}}{d\phi}+\frac{d\mathrm{V}}{d\phi}+\frac{d\mathrm{K}}{d\phi'}=0,$$
$$\vdots$$

where the several symbols are to be taken in the significations before explained.

In the particular problem, let z be measured vertically downwards from the plane of the table, then $Z=g$, and repeating for the particular case the investigation *ab initio*, the general equation of motion is

$$\Sigma\left(\frac{d^2z}{dt^2}-g\right)\delta z\, dm+\frac{d\zeta}{dt}\,\delta\zeta\,\frac{1}{dt}\,d\mu=0.$$

Let s be the length in motion, or, what is the same thing, the z coordinate of the lower extremity; and suppose also that the mass of a unit of length is taken equal to unity, we have $\delta z=\delta s$, $\frac{d^2z}{dt^2}=\frac{d^2s}{dt^2}$, $dm=dz$, and the summation or integration with respect to z is from $z=0$ to $z=s$, whence

$$\Sigma\left(\frac{d^2z}{dt^2}-g\right)\delta z\, dm=\left(\frac{d^2s}{dt^2}-g\right)\delta s\ \Sigma dz=\left(\frac{d^2s}{dt^2}-g\right)s\ \delta s\,;$$

which is of the form

$$\left(\frac{d}{dt}\frac{d\text{T}}{ds'}-\frac{d\text{T}}{ds}+\frac{d\text{V}}{ds}\right)\delta s,$$

if

$$\text{T}=\tfrac{1}{2}s'^2.\,\bar{s},\ \ \text{V}=-gs\bar{s},$$

where the bar is used to denote exemption from differentiation, but ultimately $\bar{s}$ is to be replaced by s. Considering now the second term here $\zeta=0$, but $\delta\zeta=\delta s$, and thence $\frac{d\zeta}{dt}=s'$. Moreover, $d\mu=s'dt$, and thence finally the second term is s'^2, which is of the form $\frac{d\text{K}}{ds'}$, if

$$\text{K}=\tfrac{1}{2}\bar{s'}\,.\,s'^2,$$

the bar having the same signification as before, but after the differentiation $\bar{s'}=s'$. The resulting equation is

$$\left(\frac{d^2s}{dt^2}-g\right)s+\left(\frac{ds}{dt}\right)^2=0,$$

which may be written in the form

$$s\frac{ds}{dt}\,d\left(s\frac{ds}{dt}\right)=gs^2ds,$$

and the first integral is therefore

$$\frac{sds}{\sqrt{s^3-a^3}}=\sqrt{\frac{2g}{3}}\,dt,$$

where a is the length hanging over at the commencement of the motion. If $a=0$, then the equation is

$$\frac{ds}{\sqrt{s}}=\sqrt{\frac{2g}{3}}dt,$$

and integrating from $t=0$, $2\sqrt{s}=\sqrt{\frac{2g}{3}}t$, or finally $s=\frac{1}{6}gt^2$, so that the motion is the same as that of a body falling under the influence of a constant force $\frac{1}{3}g$. It is perhaps worth noticing that the differential equation may be obtained as follows:—We have, in the first instance, a mass s moving with a velocity s', and after the particle ds $(=s'dt)$ has been set in motion, a mass $s+s'dt$ moving say with a velocity $s'+\delta s'$, whence neglecting for the moment the effect of gravity on the mass s, the momentum of the mass in motion will be constant, or we shall have

$$ss'=(s+s'dt)\ (s'+\delta s')=ss'+s'^2dt+s\delta s',$$

and therefore $s\delta s'=-s'^2dt$. Hence, adding on the right-hand side the term $gsdt$ arising from gravity, and substituting $\frac{d^2s}{dt^2}dt$ for $\delta s'$, we have the equation $s\frac{d^2s}{dt^2}=gs-\left(\frac{ds}{dt}\right)^2$ as before.

IX. "Remarks on a New Class of Alcohols." (Second Note.) By A. W. Hofmann, LL.D., F.R.S., and Auguste Cahours, F.C.S. Received May 15, 1857.

(Abstract.)

In a communication addressed to the Royal Society some time ago (Proceedings, vol. viii. No. 19), we endeavoured to delineate the characters of a new alcohol—the Allylic alcohol, which is the prototype of a new class of alcohols. We have since continued these researches in order to complete the history of this remarkable compound.

In submitting to the Royal Society the full account of our experiments upon the subject, we beg leave to mention in this abstract briefly some additional compounds which we have examined since our last communication.

Sulphide of Allyl (Garlic Oil), $C_{12} H_{10} S_2$. — When iodide of allyl is allowed to fall drop by drop into a concentrated alcoholic solution of protosulphide of potassium, a very energetic action ensues, the liquid becomes very hot, and an abundant crystalline deposit takes place of iodide of potassium. It is important that the iodide of allyl should only be added gradually to avoid spirting, by which a part of the product would be lost. As soon as the action ceases, the liquid is mixed with a slight excess of sulphide of potassium; addition of water now separates a light yellowish limpid oil possessing a strong smell of garlic. When rectified, this liquid becomes colourless, it boils at 140° C., and gives with nitrate of silver a crystalline precipitate soluble in a boiling mixture of alcohol and water, and separating from this solvent in the form of beautiful white needles, absolutely identical with those which the natural essence of garlic produces. Sulphide of allyl also comports itself with corrosive sublimate and with bichloride of platinum exactly like the natural garlic oil.

Allyl-Mercaptan, $C_6 H_6 S_2$.—When in the process for the preparation of the preceding compound the protosulphide of potassium is replaced by the hydrosulphate of sulphide of potassium, a volatile product is obtained, having a similar but more ethereal odour. This substance acts with energy upon protoxide of mercury, with which it forms a compound, dissolving in boiling alcohol, and separating from it on cooling in the form of pearly scales of remarkable brilliancy, which present the greatest resemblance to the mercaptide of mercury. The liquid obtained in the above-mentioned reaction boils at 90°, and possesses a composition and an aggregate of properties which closely resemble those of the mercaptan of the ethyl-series, being, in fact, the allylic mercaptan, $C_6 H_6 S_2$.

Allylamine, $C_6 H_7 N$.—By boiling cyanate of allyl ($C_8 H_5 NO_2$) with a concentrated solution of caustic potassa until the oily layer entirely disappears, a complete decomposition ensues, and the residue contains only a mixture of carbonate of potassium with an excess of the alkaline hydrate; the volatile products condensed in hydrochloric acid furnish a saline mass which may be obtained crystalline on evaporation. This substance is always a mixture, the constituents of which vary both in nature and properties. The action of potassa on it not only separates a base, which is readily soluble in water, but

also insoluble basic oils, the boiling-point of which rises to 180° C. Simple distillation of the liberated bases appears to give rise to decompositions, and thus to induce further complication. We have not succeeded in ascertaining with perfect precision the nature of this mixture, but we have found that it invariably contains a considerable quantity of a base which bears the same relation to allylic alcohol that is observed between ethylamine and ordinary alcohol. The formation of this substance, which we propose to designate allylamine, C_6H_7N, is perfectly analogous to the production of ethylamine by means of cyanate of ethyl.

Diallylamine, $C_{12}H_{11}N$.—Impure allylamine, obtained by the action of potassa on the cyanate, when digested with a second quantity of iodide of allyl is rapidly changed into a mass of hydriodates of new compounds. This crystalline mass evidently contains a considerable quantity of the hydriodate of diallylamine, but to succeed in separating it from this very complicated mixture would have required a more considerable quantity of substance than we had at our disposal.

Triallylamine, $C_{18}H_{15}N$.—The oxide of tetrallylammonium, to which we shall presently allude, submitted to the action of heat, is decomposed, with liberation of a basic oil. The perfect analogy of the oxide of tetrallylammonium with the corresponding ethyl-compound, leaves no doubt respecting the nature of this basic substance. It is obviously triallylamine. When saturated with hydrochloric acid and mixed with bichloride of platinum, it deposits a yellow platinum-salt, the analysis of which leads to the formula

$$\left.\begin{matrix} C_6H_5 \\ C_6H_5 \\ C_6H_5 \end{matrix}\right\} N, HCl, PtCl_2.$$

Oxide of Tetrallylammonium, $C_{24}H_{20}NO, HO$.—The chief product of the action of ammonia upon iodide of allyl is a magnificent crystalline compound, which is deposited from the solution resulting from the reaction. Iodide of allyl is rapidly attacked even by an aqueous solution of ammonia at the common temperature. By a few days' contact a large quantity of the iodide dissolves, and the solution becomes a solid mass. If no deposition of crystals take place from the solution, it is only necessary to add a concentrated solution of caustic potassa, which causes the separation of an oily layer that

speedily solidifies. The crystals thus formed are the iodide of tetrallylammonium, which, like the corresponding compound of the ethyl-series, is but slightly soluble in solution of potassa. It is obtained pure by exposing it to the air until the potassa is converted into carbonate, and then recrystallizing it from absolute alcohol.

The iodide, by treatment with oxide of silver, is immediately transformed into the oxide. This forms an alkaline solution, which possesses all the properties of the oxide of tetrethylammonium.

The iodide contains

$$\left.\begin{array}{l} C_6\,H_5 \\ C_6\,H_5 \\ C_6\,H_5 \\ C_6\,H_5 \end{array}\right\} N\,I.$$

On taking a retrospective glance at the results obtained in this inquiry, it is obvious that propylene, a homologue of olefiant gas, is susceptible of furnishing a mono-acid alcohol, which bears to it the same relation that is observed between methyl-alcohol and marsh-gas, or between benzoic alcohol and toluol, the analogue of marsh-gas among the derivatives of the aromatic acids. The hydrocarbons homologous and analogous to marsh-gas, the homologues of olefiant gas, and indeed probably a very considerable number of other hydrocarbons, may be regarded as starting-points for the production of mono-acid alcohols and of all their derivatives. All these alcohols are formed by the fixation of two equivalents of oxygen, which oxidation, however, cannot be effected directly, but is accomplished by a series of substitution-processes.

$$\underbrace{C_2\,H_4}_{\text{Marsh-gas.}} + O_2 = \underbrace{C_2\,H_4\,O_2,}_{\text{Methyl-alcohol.}}$$

$$\underbrace{C_{14}\,H_8}_{\text{Toluol.}} + O_2 = \underbrace{C_{14}\,H_8\,O_2,}_{\text{Benzoyl-alcohol.}}$$

$$\underbrace{C_6\,H_6}_{\text{Propylene.}} + O_2 = \underbrace{C_6\,H_6\,O_2,}_{\text{Allyl-alcohol.}}$$

But olefiant gas and its homologues are capable of furnishing mono-acid alcohols by another reaction, which has not yet been

applied to the series of bodies homologous to marsh-gas. In fact, we have learnt by the beautiful researches of M. Berthelot that olefiant gas and its homologues are capable of being transformed into alcohols by the absorption of two equivalents of water; olefiant gas furnishes by this reaction ordinary alcohol, and propylene, propylic alcohol; thus—

$$\underbrace{C_4H_4}_{\text{Olefiant gas.}} + 2HO = \underbrace{C_4H_6O_2}_{\text{Ethylic alcohol.}},$$

$$\underbrace{C_6H_6}_{\text{Propylene.}} + 2HO = \underbrace{C_6H_8O_2}_{\text{Propylic alcohol.}}.$$

Olefiant gas, then, and all its homologues are capable of furnishing *two* mono-acid alcohols, exhibiting slightly different features, but presenting in all their principal characters the most obvious analogy. The alcohols formed by fixation of water, such as ethylic alcohol, have long been well known; in fact, the detailed study which has been made during the last quarter of a century of most of their derivatives, has exercised the most beneficial influence upon the development of organic chemistry.

The same remark does not apply to the alcohols produced from these same hydrocarbons by fixation of oxygen. The history of allylic alcohol, which we have endeavoured to trace in the preceding pages, and to which of late important contributions have been furnished, moreover, by MM. Berthelot and De Luca and by M. Zinin, begins to throw light also upon this second group of alcohols. For this reason the discovery of allylic alcohol appears to claim the attention of chemists, even if it had not assisted in fixing, in the system of organic compounds, the exact position of several very interesting natural products, viz. the sulphuretted oils furnished by the bulbs of the garlic (*Allium sativum*) and the seeds of the black mustard (*Sinapis nigra*), which it had not been possible up to the present time to group around a similar centre.

X. "Photochemical Researches." By Prof. BUNSEN and HENRY E. ROSCOE, B.A., Ph.D. 3rd Communication. "The Optical and Chemical Extinction of the Chemical Rays." Communicated by Professor STOKES, Sec. R.S. Received May 20, 1857.

(Abstract.)

In order to determine whether the act of photochemical combination necessitates the production of a certain amount of mechanical effect, for which an equivalent quantity of light is expended, or whether this phenomenon is dependent upon a restoration of equilibrium effected without any corresponding equivalent loss of light, we must now study the phenomena occurring at the bounding surfaces, and in the interior of a medium exposed to the chemically active rays.

If I_0 represents the amount of light entering a medium, and I the amount issuing from the medium, we have $a\,I_0 = I$, when a represents the fraction of the original amount of light which passes through the medium, on the supposition that the light extinguished is proportional to the original intensity of the light. The first series of experiments was made with the view of determining this point. The intensity of the chemical rays proceeding from a constant source of light was measured before and after passage through a cylinder with plate-glass ends, filled with dry chlorine. The amount of transmitted light, I, was determined for various intensities of incident light, I_0, and the fraction $\frac{I}{I_0}$ was found to remain constant, proving that the absorption of the chemical rays varies directly as the intensity of the light. From this result, the general law of the extinction of the optical and chemical rays in transparent media may be deduced. For, as it has been shown that the amount of light transmitted through a medium of finite thickness is proportional to the intensity of the incident light, it may be assumed that this same relation will hold good for an infinitely thin medium. According to this supposition, the relation between the transmitted light, I, and the thickness of the medium, is represented by the equation $I = I_0 . 10^{-h\alpha}$ and $\alpha = \frac{1}{h} \log \left(\frac{I_0}{I}\right)$, in which I_0 represents the light before transmission, I, that after transmission through a layer of h thickness, and $\frac{1}{\alpha}$ the thickness of absorbing

medium by passing through which the amount of light has diminished to $\frac{1}{10}$th.

The difference between the incident and transmitted light, *i. e.* that lost in passing through the medium, is made up (1) of a portion reflected, and (2) of a portion absorbed or extinguished. We have experimentally determined the values of the coefficient of reflexion ζ, and the coefficient of extinction α, for the glass plates used in our cylinders. We found that 4·86 per cent. of the chemical rays, from a flame of coal-gas, which fall perpendicularly on a surface of crown glass, are lost by the first reflexion; and that the amount of light absorbed in our plates was so small as to fall within the limits of observational errors. The value of ρ for the plates of glass employed was found to be 0·0509. When the coefficient of reflexion for glass ρ is known, the amount of light a transmitted by n plates is found from the formula $\frac{1-\rho}{1+(2n-1)\rho}=a$. Hence the amount of light transmitted by two plates is 0·823. We have confirmed the accuracy of the calculated result by direct experiment, and obtained a value 0·800, or a mean of 0·811 as the coefficient of transmission of our plates.

If all the transparent media have not the same coefficient of reflexion, the order in which the media are placed will affect the amount of transmitted light. We have given an example of the mode in which the calculation must in this case be made, in the determination of the coefficient of extinction of water. We found that the amount of light absorbed by a column of water 80 millimetres thick was inappreciable. According to the method here adopted, it is possible to determine the coefficient of reflexion of all transparent fluids for the chemical rays. We have only determined the coefficient of reflexion for American mica; for the chemical rays of a coal-gas flame ρ was found to be $=0·1017$. From the coefficient of reflexion, the refractive index (i) can be calculated from the equation $\rho=\left(\frac{1-i}{1+i}\right)^2$ or $i=\frac{1-\sqrt{\rho}}{1+\sqrt{\rho}}$. The refractive index for crown glass thus calculated from $\rho=0·0509$ is found to be $i=1·583$; the refractive index for Fraunhofer's line H has been optically determined to be between 1·5466 and 1·5794 (Buff's Physik).

Another important element in the investigation of photochemical extinction is the law according to which the optical coefficient of

extinction varies with the density of the absorbing medium. A series of experiments proved that the amount of chemical rays transmitted through a medium varies proportionally with the density of the absorbing medium.

We may now proceed to the investigation of the original question proposed, viz.—in the combination of chlorine and hydrogen effected by the light, are the chemical rays expended in a relation proportional to the quantity of hydrochloric acid formed? The first point to be determined, in order to answer this question, is the coefficient of extinction of pure chlorine for the chemical rays of a coal-gas flame. The amount of light was measured before and after transmission through cylinders filled with chlorine. The loss of light by reflexion $a=0{\cdot}811$ must be deducted from the incident light, and then the coefficient of extinction for chlorine is calculated. From a series of determinations, the value of $\frac{1}{a}$, *i. e.* the depth of chlorine at 0° C. and 0·76 pressure, through which the light must pass in order to be reduced to $\frac{1}{10}$, is found to be, as a mean of five experiments, 171·7 mm. Another series of determinations were made with chlorine diluted with air, in order to prove experimentally that the absorbed light varies in the case of chlorine directly as the density. The quantity of chlorine contained was determined in each instance by a volumetric analysis. An average of six experiments gave a value for $\frac{1}{a}=174{\cdot}3$ mm. As a mean of these two series of experiments we have a value of 173·3.

If the light is not consumed in the act of photochemical change, the coefficient just found must remain unaltered when the chlorine and hydrogen mixture is employed; if on the contrary light is not only lost by the optical extinction, but an amount of light vanishes proportional to the chemical action, experiment must give a larger value for the coefficient.

In order to determine this important question we employed an apparatus (fully described in the Paper), by means of which we could expose columns of the sensitive gas of various lengths, to a constant source of light. By determining the amount of action effected in these columns of varying length, we are able to obtain the value of the coefficient of extinction for the sensitive mixture. A series of experiments showed that when the light had passed through

234 millimetres of the sensitive mixture of chlorine and hydrogen at 0° and 0·76, it was reduced to $\frac{1}{10}$ of its original intensity. If, instead of hydrogen, we had diluted the chlorine with some other transparent but chemically inactive gas, the depth to which the rays must penetrate in order to be reduced to $\frac{1}{10}$ is according to the experiments with pure chlorine, 346 millimetres. Hence it is seen that for a given amount of chemical action effected in the mixture of chlorine and hydrogen, an equivalent quantity of light is absorbed. For we find that in the case of the standard chlorine and hydrogen mixture, where, together with the optical absorption, a chemical action of the light occurs, the value of the coefficient of extinction is 0·00427; whereas in the chlorine mixture of similar dilution, where the chemical action was absent, the coefficient is found to be 0·00289, or very much smaller.

It appeared of great interest to repeat these experiments with rays from other luminous sources. For this purpose we employed the diffuse light of morning reflected from the zenith of a cloudless sky. The experiments gave a value of $\frac{1}{\beta}$ of 45·6 mm. for chlorine. That is, diffuse morning light was reduced to $\frac{1}{10}$ by passing through 45·6 millimetres of chlorine. A series of experiments made with our apparatus and the standard mixture gave a depth of 73·5 millimetres of chlorine and hydrogen before the light was reduced to $\frac{1}{10}$. From this it is seen that morning zenith light is much more easily absorbed by chlorine than lamp-light. Hence it was conjectured that the diffuse solar light might differ in its properties in this respect with the time of day or year. Experiment fully confirmed this supposition. Observations made with evening light proved that a depth of 19·7 mm. of chlorine was sufficient to reduce the light to $\frac{1}{10}$ of its original amount, and that 57·4 mm. of the standard mixture was required to effect the same end.

The conclusion which we draw from all our observations is, that the coefficients of extinction of pure chlorine for chemical rays from various sources of light are very different. The depth to which such light must penetrate chlorine at 0° and 0·76, in order to be reduced to $\frac{1}{10}$ of its original intensity, is—

(1.) For a flame of coal-gas .... 173·3 mm.
(2.) Reflected zenith light, morning .... 45·6 mm.
(3.) Reflected zenith light, evening .... 19·7 mm.

Hence it is seen that the chemical rays reflected at different times and hours not only possess quantitative but also qualitative differences, similar to the various coloured rays of the visible spectrum. Had nature endowed us with the power of discriminating the chemical rays, as we do the visible ones, by impressions of varying colour, we should see the rosy tints of morning pass in the course of the day through all the gradations of colour until the warm evening ones at length succeed.

A long and continued series of observations must be made before we are able to appreciate the influence which these qualitative differences in the chemical rays exert upon the photochemical phenomena of vegetation. That this influence must be of the greatest importance is evident from the varying effects produced in other photochemical processes by differences in the solar light. We need only mention in proof of this assertion, the fact, well known to all photographers, that the amount of light, photometrically speaking, gives no measure for the time in which a given photochemical effect is produced, and that a less intense morning light is always preferred for the preparation of pictures to a bright evening light.

XI. "On the Causes and Phenomena of the Repulsion of Water from the Feathers of Water-Fowl and the Leaves of Plants." By George Buist, D.C.L. of Bombay, F.R.S. Received April 3, 1857.

Happening to reside in Bombay, in the neighbourhood of a number of small tanks or ponds abounding with the Lotus or sacred bean of India, and with four different varieties of Water Lily, I was struck with the different appearances presented by these when immersed in water, or when water was sprinkled on them. The leaves of the lily, like those of the Lotus, floated with considerable buoyancy on the surface, but never, like the Lotus, rose above it, on a tall independent stem. The lily leaf is full of holes about the size of a pin's head, and serrated at the edges. Through these, when the leaf is pressed down, the water perforates freely. The upper surface of the leaf is smooth and shining, and water runs off it without wetting it, as it does off a piece of glass or greased surface. When

placed under the water at an angle of about 45°, the leaf of the lily seems to change colour; the dark purple leaf of the red lily appears of a bright rich pink, the dark green or bluish-green of the white, pink, and blue lilies seem to become of a bright emerald-green; the intensity of these hues varying with the angle at which the immersed leaf is seen.

When the Lotus leaf is placed under water it reflects light like a mirror, so that the image of any object, if presented to it at a proper angle, is seen by the spectator as distinctly as if the surface were one of polished metal. When water is thrown on the surface of a floating leaf, it flows off like a pool of quicksilver, reflecting light from the whole of its lower surface; and this holds good on all occasions. The repellent property of the leaf is on the upper side only, for the lower side is always wet, being only destroyed by severe rubbing. These peculiarities seem long to have been familiar to the natives, and have given rise to the Mahratta lines in reference to the virtuous man, which may be thus translated :—

> "He is not enslaved by any lust whatever;
> By the stain of passion he is not soiled,—
> As in the water, yet unwet by the water,
> Is the Lotus leaf."

On examining carefully into the cause of this, I found the Lotus leaf covered with short microscopic papillæ, which entangle the air and establish an air-plate over the whole surface, with which in reality the water never comes in contact at all. Another peculiarity connected, but not necessarily so, so far as I could discover, with this, was the singular respiratory pores of the Lotus. The leaves of the Lotus, when full-sized, are from a foot to 16 inches in diameter; on cutting off a leaf 6 inches broad, the stalk of which was less than the third of an inch in diameter, I was able to collect 33 cubic inches of air in an hour, when the vital energies of the plant must have been injured by its mutilation; at this rate a tank covered with Lotus leaves would produce daily an atmosphere 4 feet in depth throughout its whole surface. When the leaf is pushed slightly under water, a constant succession of air-bubbles seem to arise from it, at the rate of two or three a minute at each spiracle. The air-bubble diffuses itself as it is extricated, presenting a very broad base to the leaf and blunt low-crowned apex, and seems de-

tached with difficulty. The air-plate all over the surface must thus become continually renewed and the arrangement kept perfect.

Sensible respiration is not at all essential to the repelling power of leaves; the most beautiful manifestation of it I have met with is in the *Pestia,* a little floating water-plant abounding in our shallow tanks, and resembling common endive. When pushed under the surface, it looks like a little mass of burning silver. The same appearance is presented on cabbages, young clover, and a vast variety of other leaves; it is the cause of the bright pearl-lustre of dew. The same phenomenon is manifested on the wings and backs of divers when they dash into the water. In this case it has been ascribed, most erroneously as I believe, to the presence of grease or oil in the feathers, and is, I have no doubt, due to the presence of an air-plate repelling the water, so that it never comes in contact with the feathers at all. The trimming process, so carefully performed by Water Fowl, is probably an application of oil or grease, with the object of separating or dressing the little fibres of the feathers so as to produce an arrangement fitted to entangle the air. The reflexion of light from the lower surface of the water is the proof of want of contact. A piece of polished marble or of glass, a waxed, oiled or greased surface, readily throws off the water without remaining wetted; but no reflexion is in this case observable.

Might not the manufacturers of waterproof cloth or clothes take a hint on this point from the economy of nature? Could they manage to produce a surface such as would entangle and retain a film of air, no india-rubber varnish or other water-tight material would be required, while the texture would permit the free transmission of respiration or moisture from the body, which Mackintosh's and other similar contrivances obstruct.

XII. "Researches on the Phosphorus-Bases." By A. W. HOFMANN, LL.D., F.R.S., and AUGUSTE CAHOURS, F.C.S. Received June 13, 1857.

(Abstract.)

In a note on the action of chloride of methyl upon phosphide of calcium, communicated more than ten years ago to the Institute of France (Comptes Rendus, t. xxi. p. 144, ad t. xxv. p. 892), M. Paul Thénard pointed out the existence of a series of bodies which correspond to the compounds of phosphorus with hydrogen, which may, in fact, be viewed as phosphoretted hydrogens, the hydrogen of which is replaced by an equivalent quantity of methyl.

It is now many years since M. Paul Thénard abandoned the study of the phosphorus-compounds, for the first knowledge of which we are indebted to him. The unfinished state in which these researches remained, and the rich and abundant harvest collected since that period, in all the neighbouring fields of science, necessitated a revision of the subject. The discovery of methylamine, dimethylamine, and trimethylamine, and of the corresponding terms in the ethyl- and amyl-series, had shown that the hydrogen in ammonia may be replaced by binary molecules, such as methyl, ethyl, amyl and phenyl, the newly-formed compounds retaining the basic character of the original ammonia molecule; whilst the production of triethylstibine and triethylarsine had furnished the proof that the total replacement of the hydrogen in the indifferent antimonietted and arsenietted hydrogens exalts the chemical character of these compounds in a most remarkable manner, the methylated and ethylated bodies exhibiting basic characters scarcely inferior to those of ammonia itself. It remained, therefore, to be investigated whether phosphorus, which by its chemical tendencies stands between nitrogen and arsenic, would exhibit a similar deportment. It had to be ascertained in what manner the gradual entrance of binary molecules in the place of the hydrogen in phosphoretted hydrogen would change the character of the original compound. Again, the discovery of the tetrethylated ammonium-bases had opened a new field of research, in which the corresponding terms of the antimony- and arsenic-series were rapidly brought to light. It was indeed possible to predict with certainty, that an appropriately selected method would lead to the production of the analogous

derivatives of phosphoretted hydrogen. The time for resuming the study of the phosphorus-bases had in fact arrived.

We have been engaged for a considerable time in the investigation of this subject, and now beg to offer to the Royal Society in the following pages a short abstract of our results.

In the first place, we have endeavoured to obtain the bases corresponding to phosphoretted hydrogen, by a method analogous to that followed by M. Paul Thénard, simply substituting for the chloride of methyl the iodide, and for the phosphide of calcium the more energetic phosphide of sodium. After many unsuccessful experiments, however, we were compelled to abandon this plan, which is quite uncertain, unproductive, and highly dangerous.

The question then resolved itself into the discovery of a method which would yield us the desired substances conveniently, without danger, in considerable quantity, and in a state of absolute purity.

It appeared that the action of terchloride of phosphorus on zinc-methyl, zinc-ethyl, &c., the remarkable substances discovered by Dr. Frankland some years ago, would enable us to attain the contemplated result. Experiment has fully confirmed this anticipation.

The bases Me_3P and E_3P, the products of this reaction, which we propose to call respectively *trimethylphosphine* and *triethylphosphine*, remain united with chloride of zinc, and a simple dilution with an alkali is all that is necessary to liberate them.

$$3ZnCl, Me_3P + 6KO = 3KCl + 3KO, ZnO + Me_3P$$
$$3ZnCl, E_3P + 6KO = 3KCl + 3KO, ZnO + E_3P$$

They are obtained in this way as volatile oils, of a peculiar and strongly marked odour, and possessing distinctly basic properties.

We limit ourselves in this abstract to giving a synoptical table of the several compounds which have been the subject of our researches, the preparation and properties of which, together with the analytical details, are fully given in the complete paper which accompanies this note.

Methyl-series.

Trimethylphosphine	Me_3P
Platinochloride of trimethylphosphine .	$Me_3P, HCl, PtCl_2$
Binoxide of trimethylphosphine......	Me_3PO_2
Bisulphide of ditto	Me_3PS_2
Biselenide of ditto	Me_3PSe_2

Iodide of phosphomethylium........	Me_4 P I
Platinochloride of ditto	Me_4 P Cl, $PtCl_2$
Aurochloride of ditto	Me_4 P Cl, $AuCl_3$
Iodide of phosph'ethyltrimethylium ..	(Me_3 E) P I
Platinochloride of ditto ..	(Me_3 E) P Cl, $PtCl_2$
Iodide of phosph'amyltrimethylium ..	(Me_3 Ayl) P I
Platinochloride of ditto ..	(Me_3 Ayl) P Cl, PCl_2

Ethyl-series.

Triethylphosphine................,	E_3 P
Platinochloride of triethylphosphine..	E_3 P, HCl, $PtCl_2$
Binoxide of ditto ..	E_3 P O_2
Bisulphide of triethylphosphine	E_3 PS_2
Biselenide of ditto	E_3 P Se_2
Iodide of phosphethylium..........	E_4 P I
Platinochloride of ditto	E_4 P Cl, $PtCl_2$
Aurochloride of ditto	E_4 PCl, $AuCl_3$
Iodide of phosphomethyl-triethylium .	(MeE_3) P I
Platinochloride of ditto ..	(MeE_3) PCl, $PtCl_2$
Iodide of phosph'amyltriethylium....	(Ayl E_3) P I
Platinochloride of ditto	(Ayl E_3) PCl, $PtCl_2$

On glancing at the phosphorus-compounds noticed in the preceding abstract, a comparison of these substances with the corresponding terms of the nitrogen-, arsenic-, and antimony-series is unavoidably forced upon us. Whether we consider the composition, or whether we review the properties of these groups, the most striking analogies, indeed an almost perfect parallelism, cannot be mistaken; similar formulæ, similar modes of combination, similar decompositions.

This analogy is particularly manifest in the compounds belonging to the *ammonium-type*. In these remarkable bodies, nitrogen, phosphorus, arsenic, and antimony appear to play absolutely the same part. It is more especially in the oxides of these compound metals that the analogy of composition induces a perfect identity of properties, and, indeed, of very salient properties, which may be traced in almost every direction. If we were satisfied with the study of the reactions of these bodies, we should never suspect, in compounds possessing such a close similarity of properties, the

presence of elements so dissimilar as nitrogen, phosphorus, arsenic, and antimony; they might, moreover, be confounded with potassa and soda, by which they are scarcely surpassed in alkaline power.

Only the deportment of the hydrated oxides, under the influence of heat, distinguishes the derivatives of nitrogen from the corresponding terms of the phosphorus-, arsenic-, and antimony-series.

If we regard, on the other hand, the compounds belonging to the *ammonia-type*, we observe that the electro-positive character of the substances gradually rises in intensity from the nitrogen- to the antimony-compounds. Thus, trimethylamine and triethylamine are not capable of uniting with oxygen, chlorine, bromine, and iodine; a power which the corresponding terms of the phosphorus-, arsenic-, and antimony-series possess in a high degree.

Triethylamine unites with the acids producing compounds of the ammonium-type, and containing

$$E_3\,N, HCl$$
$$E_3\,N, HSO_4$$
$$E_3\,N, HNO_6$$

The corresponding compounds in the arsenic- and antimony-series do not exist; at all events chemists have not yet succeeded in preparing them. Triethylarsine and triethylstibine combine only directly with oxygen, chlorine, sulphur, &c., producing saline bodies which have the composition respectively,—

$$E_3\,AsO_2 \;\ldots\ldots\ldots\ldots\; E_3\,SbO_2$$
$$E_3\,AsCl_2 \;\ldots\ldots\ldots\ldots\; E_3\,SbCl_2$$
$$E_3\,AsS_2 \;\ldots\ldots\ldots\ldots\; E_3\,SbS_2$$

In the phosphorus-series, lastly, the two classes are represented. Triethylphosphine not only forms compounds analogous to the salts of triethylamine, but also the terms corresponding to the binoxides of triethylarsine and triethylstibine. We have in the first place the terms

$$E_3\,P, HCl$$
$$E_3\,P, HSO_4$$
$$E_3\,P, HNO_6,$$

and in the second place compounds of the formula

$$E_3\,PO_2$$
$$E_3\,PCl_2$$
$$E_3\,PS_2$$

The phosphorus-compounds accordingly hold a position intermediate between the nitrogen-compounds on the one hand, and the arsenic- and antimony-series on the other. It cannot, however, be denied that the phosphorus-bodies exhibit rather a tendency towards the arsenic- and antimony-series, than towards the nitrogen-group.

This cannot surprise us, when we consider the close analogies which phosphorus and arsenic present in many other directions. Both phosphorus and arsenic form well-characterized polybasic acids; the acids of antimony are not yet sufficiently investigated, but the acids of nitrogen, which are better examined, are all found to be monobasic. The equivalent numbers, too, of phosphorus, arsenic, and antimony, present a remarkable connexion, the difference between those of phosphorus and arsenic, and those of arsenic and antimony being virtually the same—

$$\left.\begin{array}{ll}\text{Phosphorus} \ldots\ldots\ldots & 31 \\ \text{Arsenic} \ldots\ldots\ldots & 75\end{array}\right\}\text{difference } 44,$$
$$\left.\begin{array}{ll}\text{Arsenic} \ldots\ldots\ldots & 75 \\ \text{Antimony} \ldots\ldots\ldots & 120\end{array}\right\}\text{difference } 45,$$

whilst the equivalent of nitrogen stands altogether apart from the rest.

The same relative position of the elements nitrogen, phosphorus, arsenic, and antimony, may also be traced in their hydrides,

$$H_3N \quad H_3P \quad H_3As \quad H_3Sb.$$

Ammonia is a powerful alkali; phosphoretted hydrogen unites only with hydrobromic and hydriodic acids, whilst in arsenietted and antimonietted hydrogen, the power of combining with acids has altogether disappeared. In these hydrogen-compounds the gradation of properties is indeed even more marked than in their trimethylated and triethylated derivatives. On comparing the terminal points of the series, ammonia and antimonietted hydrogen, we cannot fail to be struck by the dissimilarity of properties which at the first glance appears to limit the analogy of the two compounds to a mere parallelism of composition.

In the methylated and ethylated derivatives of these compounds, the intensity of the chemical tendencies in general is so much raised, that the gradation is no longer perceptible to the same extent.

XIII. "Report of Proceedings of the Astronomical Expedition to Teneriffe, in 1857." By Prof. C. PIAZZI SMYTH. Presented by G. B. AIRY, Esq., F.R.S., Astronomer Royal. Received June 2, 1857.

(Abstract.)

The Report, presented originally to the Admiralty, at whose expense and whose orders the Expedition was sent out, consists of ten parts, with their contents as follows:—

Part 1. Astronomical and Physical observations.

Part 2. Mountain Meteorological Journal.

Part 3. Reductions of above.

Part 4. Sea-level Meteorological Journal.

Part 5. Reductions of above.

Part 6. Plans, Sections, and Astronomical illustrations.

Parts 7, 8, 9. Condensed statement of results and conclusions in Astronomy, Physics, Meteorology, Geology, Botany, and Navigation.

Part 10. Photographs illustrating the botany and geology of three different levels in the Island.

The original instructions of Prof. Piazzi Smyth had been purely astronomical, and were to ascertain how much telescopic vision could be improved by eliminating the lower third of the atmosphere. In furtherance of this view, he erected the Sheepshanks equatorial of the Edinburgh Observatory on Mount Guajara in Teneriffe at a height of 8903 feet, and found the space-penetrating power extended from mag. 10 to mag. 14, and so great an improvement in definition, that a magnifying power of 240 could be used with more satisfaction on the mountain, than one of 60 in Edinburgh. After a month's experience of this station, he ascended to a higher one—the Alta Vista—at a height of 10,702 feet on the eastern slope of the Peak, and there erected the large equatorial of Cooke, lent for the occasion by Mr. Pattinson of Newcastle. The definition was here admirable, and the telescope equal, if not superior, to all the test objects it was turned upon. A comparative hypsometric estimate was not possible; for although the observer had spent an equal number of days to what he employed on the Peak, in trying to ascertain the capabilities of the instrument at the house of its hospi-

table owner, on several visits, the cloudy atmosphere of Newcastle had always prevented any good observations being made.

An argument in favour, however, of the increase of height was ascertained through means of radiation instruments, which indicated almost as much improvement between 10,700 and 8900 feet, as between the latter height and the sea-level. This unexpected degree is attributed by the author to the circumstance of the highest station being almost clear of certain dry, hazy, or dusty strata of atmosphere, which include and overtop the intermediate station, and are, next to the clouds themselves, the greatest obstacle to good telescopic vision of the heavenly bodies. The results with regard to the clouds were equally satisfactory, for $\frac{7}{10}$ths of the whole were found to be under the level of 5000 feet.

The author expresses the most cordial thanks to Sir Charles Wood, the First Lord of the Admiralty, for his intelligent liberality, to which the Expedition owed its existence; also to Lord Clarendon, the Secretary of State for Foreign Affairs, for his letters to the Spanish Government and to the English Customs; and he acknowledges with pleasure the amount of assistance which he received from many private gentlemen, friends of science, through whose contributions he was enabled to execute, with a limited grant, and in a circumscribed portion of time, a much greater amount of work than he could otherwise have hoped for; those gentlemen were, Robert Stephenson, Esq., M.P., G. B. Airy, Esq., H. L. Pattinson, Esq., Dr. J. Lee, Prof. Stokes, the late Admiral Beechey, Admirals FitzRoy, Manners and Smyth, Prof. Baden Powell, J. Gassiot, Esq., Capt. Washington, and Messrs. Cooke and Adie.

In the island of Teneriffe, Prof. Piazzi Smyth also received valuable aid in many ways from C. Smith, Esq., of Orotava, formerly of Trinity College, Cambridge, L. Hamilton, Esq., of Santa Cruz, Don Francisco Aquilan, Spanish engineer, and more particularly from the acting Vice-Consul of Orotava, Andrew Goodall, Esq., and his nephew Mr. Carpenter, who were aiding and abetting, and in fact forming an important part of the Expedition during its whole stay in Teneriffe.

XIV. "Account of the Construction of the New National Standard of Length, and of its principal Copies." By G. B. Airy, Esq., F.R.S., Astronomer Royal. Received May 2, 1857.

(Abstract.)

The author premises that the work to which this account relates was executed almost entirely by Mr. Baily and Mr. Sheepshanks. He then proceeds with the Account, which is divided under nine Sections.

Section I. contains the History of the British and of some Foreign Standards, and of the methods of using them in Base-measures and Pendulum-measures, anterior to the legalization of the Imperial Standards by the Act of Parliament of 1824; the definition of the Standard of Length by that Act; and the provision for its restoration in case of loss. The first record cited is that of the laying down of the English Standard Yard on the Royal Society's brass bar, described in the Philosophical Transactions, 1742-3; then allusion is made to the comparisons by Graham, Maskelyne, Ray, Shuckburgh, those of the Base du Système Métrique, and those by Kater (1818 and 1821). The Sections of the Act of Parliament of 1824, which define the British Yard and prescribe the method for restoring it in case of loss, by reference to the length of the Seconds' Pendulum, are cited.

Section II. gives some description of the Comparisons of Standards made between the passing of the Act of 1824 and the appointment of a Commission for consideration of Standards after the destruction of the Imperial Standard in 1834; with remarks suggested by the advance of collateral theory and experiment in that interval. Extracts are given from Kater's papers of 1826, 1830, and 1831, in which new difficulties were described and new constructions planned to obviate them. Allusion is made to the characteristics of the principal Base-measures in Britain and on the Continent. The constructions of the Ordnance Standard Bars and of the Royal Astronomical Society's Tubular Scale are described, and reference is made to the accounts of their comparisons. Bessel's measure of the seconds' pendulum, Bessel's construction of the Prussian 3-foot

standard, and Bessel's and Sabine's investigation of the atmospheric correction to the vibrations of the pendulum, are noticed. The result of the Astronomer Royal's inquiry into the points of support of a bar proper for preventing extension of its upper surface by its flexure is given. The state of the science of Standards in the year 1834 is then described as follows. It had been shown that it was imprudent to trust to points or lines traced on the surface of a bar, and therefore, supposing the Standard to be a line-measure, only two lines or points ought to be used, sunk to the middle of the bar's thickness. It had been shown that it was imprudent to lay the bar upon a table or upon fixed supports of any kind, and therefore the bar must be stiff enough to bear to be supported upon a few points at which rollers could be conveniently applied. It had been shown that the physical reference provided in the Act of Parliament of 1824 was erroneous in one particular and doubtful in another; and, as it seemed likely that similar uncertainties might be found in any other physical reference, the conviction was gradually rising that it would be better to trust, for restoration, to attested copies of the Standard. The question of the propriety of adopting line-measure or end-measure for the National Standard, which in this country had been practically decided (without a single opposing instance as regarded accurate standards) in favour of line-measure, had again been raised by Bessel's adoption of end-measure.

On 1834, October 16, occurred the fire at the Houses of Parliament, in which the Standards were destroyed.

Section III. records the appointment of the Treasury Commission of 1838, its proceedings and Report; the appointment of the Treasury Commission of 1843, and its proceedings to the death of Mr. Baily in 1844. The Report of the first Commission recommended the adoption of a material Standard, without any reference to physical experiment; and recommended that four copies should be made, of which one should be immured in a wall of a public building, that the new Standard should, by means of bars which had been compared with the old Standard, be made as nearly as possible equal in length to the old Standard, and that the superintendence of the construction should be entrusted to a Committee. These recommendations were adopted by the Lords Commissioners of the Treasury, and led to the appointment of the second or Superintending Committee, and

to the appointment of Mr. Baily as immediate manager of the work. Mr. Baily made experiments on the fitness of different alloys, and fixed upon a hard bronze or gun-metal as best for the Standards. He then repeated some of Kater's experiments; made experiments on the thermometrical expansion of different metals; compared the various bars on which the restoration of the Standard must depend; and proved that the Royal Astronomical Society's tubular scale was not worthy of entire credit as a means of restoring the length of the old Standard. Mr. Baily's death interrupted these inquiries. Generally, however, it appeared that it would be very undesirable to refer in any degree to Shuckburgh's scale (adopted by Kater as the Scientific Standard), inasmuch as there was no security whatever that, in retaining documentary or numerical expressions of measure founded on this scale, we were referring to a consistent system; that the old legal Standard was, through a sensible range, indeterminate; that the new Standard must be firm in its structure; that firm comparing-apparatus must be used, and that new thermometers must be constructed.

Section IV. records the proceedings of the Committee and of Mr. Sheepshanks (who, after the death of Mr. Baily, undertook the construction of the Standard of Length) to June 1847; the construction of new Thermometers; the erection of the massive Comparing Apparatus in the cellar of the Royal Astronomical Society at Somerset House, with a description of the Apparatus and of Mr. Sheepshanks's method of comparing.

Section V. describes the proceedings of Mr. Sheepshanks to the middle of 1850; the preparations of Thermometers; the discussion of the values of the scales compared with the old Standard; the successive adoption and rejection of "Brass 2," "Split-plug A," and "Bronze 12," as Basis for the new Standard; the final adoption of "Bronze 28;" experiments on thermometric expansion; and first suspicion of personal equation.

Section VI. gives an account of the operations of important character to the end of 1853; comparisons of a small number of bars with Bronze 28; investigation of personal equation; investigation of the effect of inside or outside position of the bar; investigation of the relative thermometric expansion of steel, wrought iron, cast iron, copper, and brass, as compared with bronze; trial and rejection of

Baily's apparatus. The whole of this work is of the most elaborate kind.

Section VII. gives the results of comparisons of numerous bars, from 1851 to 1855, and describes the grounds for suspicion of change in Bronze 28, and the removal of the suspicion. Mr. Sheepshanks's death occurred at the end of the observations relating to the suspected change; and the operations on bars, defining the yard by line-measure, were closed.

Section VIII. gives an account of the formation of End-measure Bars, which had been begun by Mr. Sheepshanks, but was completed by Mr. Simms, under the superintendence of the Astronomer Royal. The general principle is this. If two end-bars have each a defining mark almost equally distant, in the two bars, from the middle of its length; and if the two bars are placed end to end, the longer segment of the one touching the shorter segment of the other, the distance between the two lines can be compared, by microscopes, with a line-standard. If the contacts be now made by the other ends, a similar comparison can be made. If the two results be added together, we have a comparison of the sum of the entire lengths of the two end-standards with double the length of the line-standard. This operation being performed, so as to effect a comparison of the three pairs which can be made from three end-standards (the sum of each pair being compared with the double line-standard), we have three simple equations from which the lengths of the three end-standards can be deduced. The end-bars are constructed, some of bronze, some of iron or steel; but in all, the ends are of agate, ground to the curvature of a large sphere, whose centre is the middle point of the bar. The lengths of three bronze end-bars, and of four iron or steel end-bars, were determined by this process.

Section IX. gives a statement of the closing proceedings of official character, with extracts from the Final Report of the Commission, and extracts from the Act of Parliament legalizing the new Standard; a table of standard temperatures for the compared bars; and an account of the disposal of the bars. The Act of Parliament (18° and 19° Victoriæ, cap. 72) recognizes the Bar deposited at the Exchequer Office, and numbered 1, as bearing "the genuine Standard of that measure of length called a Yard," and recognizes four copies as available for restoration of the Standard in case of loss. These copies

are: No. 2, deposited at the Royal Mint; No. 3, in charge of the Royal Society; No. 4, immured in the Cill of the Recess on the East Side of the Lower Waiting Hall in the New Palace at Westminster; and No. 5, deposited at the Royal Observatory, Greenwich.

The whole number of bars accurately compared is 78. Of these, four tubular scales were not the property of the British Government; seven are end-measures; all the remainder are line-measures. They have been distributed liberally to foreign Governments and to British Offices; several, however, remain at the Royal Observatory, Greenwich, still disposable.

The whole of the documents relating to the preparation and comparison of the Standards are preserved at the Royal Observatory.

XV. "On the existence of the Decidua around the Ovum within the Fallopian Tube, in four Cases of Fallopian-Tube Conception, and on the absence of any trace of Decidua in the Cavity of the Uterus in the same Cases." By Robert Lee, M.D., F.R.S., Fellow of the Royal College of Physicians, London. Received May 28, 1857.

(Abstract.)

The author observes that more than two hundred years have elapsed since Riolan published a case of Fallopian-tube gestation, and that numerous cases have since been recorded in which the human ovum, after impregnation, instead of passing into the cavity of the uterus, has been arrested in the canal of the tube, and sudden death taken place from rupture of its coats and hemorrhage into the sac of the peritoneum. In none of these cases has a minute anatomical examination been made of the ova thus found in the Fallopian tubes, with the view of determining whether they have the same structure as ova found within the cavity of the uterus, or expelled from it prematurely in a healthy condition.

After referring to cases of Fallopian-tube conception published by Drs. Baillie, Denman, and J. Clarke, Mr. Langstaff, M. Breschet, and Dr. Elliotson, the author gives the details of four cases, in all of which there was no decidua found within the uterus, but the decidua,

consisting of two layers corresponding with those usually termed decidua vera and reflexa, was found in the tube, adhering to its inner surface and surrounding the placenta and villi of the chorion. The following is the description given by the author of the appearances observed in the last of these cases:—"The uterus was enlarged, and the whole lining membrane coated with a thick irregular layer of a substance resembling the fibrine of the blood, of a red colour, in the upper part. The right Fallopian tube about the middle was as large as a walnut, or larger where its coats had burst, and a coagulum of blood was hanging through the irregular aperture. The tube was pervious from the corpus fimbriatum to the dilated part. On cutting open this expanded portion, a small embryo enclosed in the amnion was observed, and the vesicula umbilicalis, remarkably large, with its peduncle, came into view. All the cells of the placenta and villi of the chorion were seen distended with coagulated blood and surrounded with a deciduous membrane, a great part of which has been separated from the inner surface of the tube."

XVI. "Experimental Researches on the Conductive Powers of various Substances, with the application of the Results to the Problem of Terrestrial Temperature." By WILLIAM HOPKINS, Esq., M.A., F.R.S., of St. Peter's College, Cambridge. Received June 10, 1857.

(Abstract.)

1. The author remarks, that in giving an account of these experimental researches, it is first necessary to define strictly the manner in which the *conductivity* or *conducting power* of a substance with reference to heat, is accurately *measured*. For this purpose, conceive the conducting substance to be bounded by two parallel plane surfaces of indefinite extent, the distance between them being h. Suppose one of these bounding surfaces (which, for convenience, may be called the *lower* one) to be kept at a uniform and constant temperature t_1; let the temperature of the *upper* surface be also constant and uniform, and equal to t_2; and let τ denote the temperature of the free space into which the heat radiates from the *upper*

surface. Then, if we denote the *conducting power* of the substance by k, and the *radiating power* of its upper surface by p, we obtain by mathematical investigation,

$$\frac{k}{p}=\frac{t_2-\tau}{t_1-t_2}h.$$

It is here supposed that k is independent of the temperature of the substance, and that p is equally independent of that of the surface from which the radiation takes place. It may also be remarked, that the quantity of heat which radiates from a unit of area in a unit of time, is measured by the product of p, and the difference of the temperatures, t_2 and τ, of the radiating surface and surrounding medium. It is the ratio $\left(\frac{k}{p}\right)$ which the conducting bears to the radiating power, which has more frequently been determined in researches of this kind; but this would not have sufficed for the author's object, which has been the determination of the values of k for different substances. The radiating power (p) probably varies for different substances as much as the conductive power (k), but all consideration of the former power will be avoided if we suppose the radiating surface of the substance to be covered with a thin layer of some given substance which shall take the temperature of the upper surface of the substance itself, and from which the radiation shall always take place, whatever be the nature of the substance experimented on. Thus if c denote the radiating power of the superimposed thin layer (which was mercury in these experiments), we shall have

$$\frac{k}{c}=\frac{t_2-\tau}{t_1-t_2}h;$$

a formula which (c being always the same) enables us to compare the conducting powers for different substances, or to determine their absolute numerical values when that of c is once determined. In the actual experiments some error was necessarily superinduced by the necessity of working with portions of the different substances of comparatively small instead of indefinitely large horizontal extent, such as strict mathematical accuracy would require. This error, however, was undoubtedly small, and, moreover, can have had extremely little effect on the *relative* values of k, since it must have

been nearly the same for all the substances on which the experiments were made.

The apparatus made use of was sufficiently simple. The heat was derived from a stove, the fire within which could be elevated, depressed, or entirely withdrawn at pleasure. A very shallow pan of mercury was placed over the stove, the fire being so regulated as to preserve the mercury at any constant required temperature. A cylindrical block of any substance, the conductive power of which was to be determined, was so placed as to rest with its base just in contact with this mercury, from which it derived its temperature (t_1). Its upper end was also covered with sufficient mercury just to cover the small bulb of a thermometer. The temperature of this latter mercury gave t_2. Careful arrangements were made for observing these temperatures, as well as that of the air into which the heat radiated from the upper mercury. Precautions were also taken to prevent the lateral transference of heat through the sides of the block, and any influence of radiation from the heated stove which might affect the results of the experiments. When the temperature (t_2) of the upper mercury became stationary, the experiment was completed, and the substitution of this stationary value of t_2, together with the values of t_1 and τ in the above formula, gave the numerical results required.

2. The following were some of the results obtained for conductive powers as measured by the ratio $\frac{k}{c}$:—

Chalk............	·056
Clay	·07
Sand	·15
Sand and clay	·11.

These substances were all in the state of *very dry powder*. In the last case the sand and clay were in equal quantities.

Substances in the state of rock-masses.

(1) *Calcareous rocks.*

Chalk (same block from a dry state to a state of saturation with water) from	·17 to ·30
Oolites from Ancaster (dry to saturated)	·30 to ·40
Hard compact limestones	·50 to ·55

(2) *Argillaceous substances.*	
Clay, very dry to very moist	·23 to ·37
(3) *Siliceous rocks.*	
New red sandstone (same block dry to saturated)	·25 to ·60
Freestone	·33 to ·45
Hard compact sandstones (Millstone-grit)	·51 to ·76
(4) Hard, compact, old sedimentary rocks	·50 to ·61
(5) Igneous rocks	·53 to 1·00

Effect of Pressure.

3. This effect was not appreciable for a pressure of 7500 lbs. per square inch in such substances as bees'-wax and spermaceti. Nor was there any sensible effect with chalk between a pressure of 4300 lbs. and 7500 lbs. per square inch.

Clay which when incompressed had a conducting power $=\cdot 26$, had when compressed with 7500 lbs. per inch, a power $=\cdot 33$; and the conducting power of a mixture of sand and clay in equal quantities rose from ·36 to ·378 by an increase of pressure from 4300 lbs. to 7500 lbs. per inch.

Generally the effect of pressure is much less than might have been anticipated.

Effect of Discontinuity.

4. When the conducting mass consists of a number of strata superimposed on each other, the mathematical problem presented to us requires a distinct investigation, which is here given under a very general form, together with the experiments necessary to determine the effect of this kind of discontinuity. The result is that if a mass of sandstone consisted of a number of strata, the conducting powers of which should be about ·5, the mean conductivity of the whole would not be diminished by more than about $\frac{1}{20}$th part, supposing the average thickness of strata to be 1 foot; or by about $\frac{1}{10}$th, if that average thickness should be 6 inches. This effect is much less than might possibly have been anticipated.

Effect of Moisture.

5. This effect was very considerable in those rocks which are great absorbents of water. The maximum effect appears to be produced

by a quantity of moisture which falls considerably short of producing complete saturation. The conducting power of a piece of dried chalk was $=\cdot 19$, but became $=\cdot 30$ when the substance was very moist. That of a well-dried piece of new red sandstone was $=\cdot 25$, but became as much as $\cdot 60$ when saturated. Both these substances absorbed a large quantity of water. Ancaster oolites absorbed considerably less, and their conductivity was affected in a smaller degree. For a block of dry clay the conductive power was $\cdot 23$, and became $\cdot 37$ when well moistened. Close indurated sandstone, palæozoic rocks of close texture, and igneous rocks are bad absorbents, and are very little affected in their conductive powers by moisture.

Comparison of Deductions from Theories of Terrestrial Temperature with the Results of Observation.

6. It has long been established by mathematical investigation, that if a large globe like the Earth be heated in any manner and in any degree, its temperature at points not too remote from its surface, and after a sufficient lapse of time, will necessarily become such that the increase of temperature in descending along a vertical line will be proportional to the increase of depth. In this enunciation, however, it is assumed that the conductive power throughout the mass, or at least throughout its more external portion, is uniform. The difference of conductive power between the unstratified and sedimentary portion of the earth's crust, or that between one sedimentary portion and another, has not hitherto been taken into account*. The author has investigated the problem assuming the crust of the globe to consist of any number of strata of different conductive powers and bounded by parallel surfaces, the problem being much simplified by considering their surfaces as plane instead of spherical. Then, assuming the temperature of the crust of the globe to be due entirely to the transference of heat from its central portions to its surface, it is shown that the increase of temperature in descending vertically through any two strata, ought to be in the inverse ratio of the conductive powers of those strata, whether the two strata belong to the same group of stratified beds, or to two different groups in different localities. Such at least must be the result unless we introduce very

* Except in the case in which Poisson investigates the state of temperature of a sphere surrounded by a single concentric spherical shell of different conductivity.

arbitrary and, as the author conceives, entirely inadmissible hypotheses into the problem.

For the purpose of testing this theory in its application to our own globe, four or five cases of Artesian wells and vertical shafts are especially referred to, in which the temperature has been carefully observed at greater depths than at any other places in Western Europe, or probably in any other part of the globe*. The cases spoken of are the following:—

(1) An Artesian well near Geneva.—Depth = 225 metres; increase of depth for 1° (F.) = 55 feet.

(2) An Artesian well at Mondorff in the Grand Duchy of Luxembourg.—Depth = 730 metres; increase of depth for 1° (F.) = 57 feet.

(3) An Artesian well at New-Saltzwerk in Westphalia.—Depth = 644·5 metres; increase of depth for 1° (F.) = 54 feet.

(4) The Puis de Grenelle at Paris.—Depth = 546 metres; increase of depth for 1° (F.) = 60 feet.

(5) A coal shaft at Duckenfield, near Manchester.—Depth = 1400 feet; increase of depth for 1° (F.) = 65 feet.

(6) A coal shaft at Monkwearmouth.—Depth about 1700 or 1800 feet; increase of depth for 1° (F.) about = 60 feet.

The general rate of increase of temperature in our own deeper coal-mines is that of about 1° (F.) for 60 feet in depth; and the same result has been obtained for many parts of the chalk in Northern France.

These cases present a remarkable approximation to uniformity, whereas the conductive powers of the strata which have been penetrated are very different. Cases (4) and (5) present the best means of comparison. The Puis de Grenelle passes through nearly 500 metres of chalk, the conducting power of which is estimated by the author at somewhat more than ·25, while the mean conducting power of the rocks through which the coal shaft at Duckenfield passes, is estimated, by means of experiments performed on specimens of these rocks, at rather more than ·5. This is about twice as much as in the former case, whereas the depths corresponding to the same increase of temperature are only as 65 to 60, instead of being in the ratio of about 65 to 35, as they ought to be according to the

* In a great majority of instances observations of this kind have not been made with sufficient care to be relied on.

theory here considered. In all the other cases the conductive powers of the masses penetrated are doubtless greater than that of the chalk at Paris, though, for the most part, they present a *more* rapid increase of temperature in descending, instead of a *less* rapid increase (as this theory would prescribe) than the Puis de Grenelle.

Within the region comprising the cases above cited, there are many local variations as to the rate of increase of terrestrial temperature in descending below the earth's surface. The author conceives that these phenomena cannot be accounted for according to this theory without the introduction of arbitrary hypotheses.

Upon the whole, he believes that in the present state of our knowledge of terrestrial temperature, it is impossible to account for its phenomena by regarding them as the consequence simply of heat, not generated in, but transmitted through the crust of the globe from some deep-seated central source.

The discrepancy between the actual terrestrial temperatures and those which would be assigned by the theory here discussed, may be illustrated perhaps by placing the subject in a rather different point of view. It is assumed in the theoretical investigation, that the isothermal surfaces at depths sufficiently great (as 50 or 100 miles for example) are approximately concentric with the earth's external surface, or, speaking with reference to areas not too large, parallel to that surface, in which case it is proved that the isothermal surfaces at comparatively small depths (not much exceeding that of the sedimentary beds) cannot be parallel to the external surface. For example, the depth of an isothermal surface of given temperature, which should be some 3000 feet at the Puis de Grenelle, ought to be nearly 6000 feet at the coal shaft at Duckenfield; and at other places it ought to be very nearly proportional to the conductive power of the terrestrial mass lying above it. But the observations above cited demonstrate that, independently of local irregularities, such an isothermal surface is nearly at equal depths throughout the whole region of Western Europe.

No theory of terrestrial temperature, then, can meet the requirements of observation which does not account for isothermal surfaces approximately parallel (with local variations) to the earth's external surface at comparatively small depths beneath it. Moreover, it is easily shown that the quantity of heat transmitted from such a surface to the external surface, must be proportional to the conductive power

of the superincumbent mass through which the transmission takes place (in the previous case the quantity of transmitted heat is independent of that power). Consequently, whatever may be the cause supplying the heat at depths not much exceeding the general aggregate depth of the sedimentary beds, it must furnish a *quantity of heat* proportional to the vertical flow of heat, *i. e.* a quantity proportional to the conductive power of the superincumbent mass. Thus the energy of the producing cause must have distinct relations to *superficial conditions*. Must not, then, the cause itself be at least partly *superficial*, and not entirely *central*? The author is convinced that such must be the case. He does not profess, in this paper, to carry his speculations further.

It should be remarked that the argument derived from the above investigations is not directly against the theory of a *primitive* heat, but only against the manifestation of the remains of such heat as the sole cause of existing terrestrial temperatures in the superficial crust of the globe, at depths beyond the sensible effect of the direct solar heat. Whatever may be the weight of the argument in favour of the earth's original fluidity (and therefore of its primitive heat), founded on the oblateness of its form, for example, the cogency of such argument remains unaltered. At the same time, all the collateral arguments in favour of primitive heat, founded on the existing temperature of the earth's crust, or the climatal changes which are believed to have taken place on its surface, are deprived, the author conceives, of nearly all their weight. Moreover, admitting only a part of the existing terrestrial heat to be due to superficial causes, the flow of heat from the earth's central portions must be less by that amount than if the whole flow were due to central heat. Consequently the rate of increase of terrestrial temperature *due to the flow of central heat* must be proportionally diminished, and the depth at which we should arrive at the temperature of fusion proportionally increased. The conclusion, therefore, that the earth's solid crust is as thin as some geologists have supposed it to be, as well as all theories based on that conclusion—whether of volcanic action, or of elevation and depression of the earth's surface—must be deprived of nearly all their force.

The remainder of this paper contains details of experiments, and descriptions of the apparatus used in making them.

XVII. "On the Perihelia and Nodes of the Planets." By EDWARD J. COOPER, Esq., F.R.S. (Second Communication.) Received June 10, 1857.

Early in the year 1855 I had the honour to transmit to the Royal Society a paper on the distribution of the perihelia and ascending nodes of the then discovered planets, which was read at the meeting of the Society held on the 8th of March in that year. In that paper I called attention to my first notice of the phenomena in the Preface to my little work on Cometic Orbits.

Ten asteroids having been since added to the number, I requested my first assistant, Mr. Graham, to include them in a new distribution of the perihelia and nodes, and he has just reported to me the results. Instead, however, of following precisely the same semicircles which I adopted, he referred in the first instance to the larger planets, with a view to ascertain whether or not in the small planets there were an apparent preference for the heliocentric semicircles in which the perihelia and nodes of the majority of the larger planets are found. Thus,—

For large Planets.

Longitude of Perihelion, 1856·0.	Longitude of ascending Nodes.
7 from 12° 1′ to 168° 19′	7 from 46° 36′ to 130° 12′
1 at 333° 24′	0 elsewhere

Middle point of Arc containing greatest number.

90° 10′	88° 24′

Mean of these........ 89° 17′

Taking this in round numbers=90°, and dividing the asteroids into three groups in the order of their discovery, we have

	Perihelia.			Ascending Nodes.	
	0° to 180°.	180° to 360°.		0° to 180°.	180° to 360°.
14	10	4	14	11	3
14	10	4	14	10	4
15	9	6	15	7	8
43	29	14	43	28	15
Large 8	7	1	7	7	0
51	36	15	50	35	15

In the case of the perihelia no other two semicircles give a greater disproportion between the numbers. The semicircle 355° to 175° contains 37 of the nodes, the opposite one 13.

But in addition to this development of my original plan regarding the heliocentric longitudes of the perihelia and nodes, Mr. Graham has found a remarkable coincidence between the foregoing numbers and the periods of the discovery of the small planets. It appears that 28 have been detected between the vernal and autumnal equinoxes, and only 15 in the other half-year.

He states that this circumstance, which at a first glance might seem to throw some light upon the facts, proves, after a moment's consideration, the exact opposite to what might have been expected, at least in its bearing on the perihelia, for

	180° to 360°.	0° to 100°.
Longitudes of Aphelia	29	14
Longitudes of descending Nodes ..	28	15
Point of Ecliptic in opposition at date of discovery	28	15

"If, then," Mr. Graham adds, "there be any connexion between these results, it is not easy to imagine why discoveries should be more frequent near the *descending* node; and it is quite contradictory that there should be a greater facility of finding the planets in the more remote parts of their orbits." Upon these facts I abstain from making any comment, excepting that the present data tend to strengthen the conviction that some physical cause, as yet unapplied to these phenomena, may be in operation. Appended to this paper are two diagrams, bringing before the eye more clearly than numbers, the heliocentric places of the perihelia and nodes which are the subjects of this notice.

XVIII. "On the Development of *Carcinus Mænas.*" By Spence Bate, Esq., F.L.S. Communicated by Sir W. Snow Harris, F.R.S. Received May 1st 1857.

(Abstract.)

The author, after noticing the history of the subject, and the

opposition which the assertion, "that the *Zoë* of naturalists is the larva of a common crab," received, traces the progress of the development of the animal from the *Zœa* to the adult, and endeavours to demonstrate, that from the youngest to the most perfect form, the changes are the result of no sudden transformation, but produced by a gradual series of alterations contemporary with every succeeding moult; that the *Zœa* is connected with the *Megalopa*, and the latter with the adult by many intermediate gradations, each in itself scarcely appreciable, and progressively approximating nearer and nearer the more perfect stages.

The author asserts that the development is earliest and most complete anteriorly; that when first born, the seventh or posterior segment of the head, one or more of the posterior segments of the *pereion* (thorax), and the penultimate of the *pleon* (abdomen) are wanting in the brachyurous Decapods; but that this general law loses somewhat of its force in the descending scale of development; and as it becomes less persistent, the animal approximates in the larval condition nearer to the form of the adult type; while on the other hand, the same appears to be a constant law of the depreciation in adult forms, as exhibited in the more or less aberrant Amphipoda, such as *Cyrtophium, Dulichia*, &c. The author likewise shows that the appendages, which act the principal parts in the larvæ, become the secondary parts of the same organs in the perfect animal. For instance, the lower antenna is represented in the larva by the complementary appendage of the adult form; the true antenna is developed from the base of the embryonic organ, which represents the squamiform and spinous appendages, more or less constant in the mucrourous Decapods, but lost in the short-tailed genera, and the organ itself is gradually increased with every successive moult. This is true, more or less perfectly, of all the other appendages present in the larvæ of all Decapoda; and no change of form, as understood in the term metamorphosis as applied to insects, takes place in the development of *Carcinus*. That the distance between the old and young forms is the result of an exaggeration of parts in the larva as compared with the relative proportion of the same in adult animals, together with the absence of others, which are gradually produced, and assume the permanent condition of the adult type.

The author has observed the rudiments of the future legs shortly

after birth. He has dissected and figured eight or nine of the more important stages, and shown the relative alteration of each part consecutively, commencing with the *Zœa* taken from the egg, and pursued the observations through the older forms to that of the adult *Carcinus*.

The paper is carefully illustrated by drawings made by the author.

XIX. "On the Electro-dynamic Qualities of Metals:—Effects of Magnetization on the Electric Conductivity of Nickel and of Iron." By Professor W. Thomson, F.R.S. Received June 18, 1857.

I have already communicated to the Royal Society a description of experiments by which I found that iron, when subjected to magnetic force, acquires an increase of resistance to the conduction of electricity along, and a diminution of resistance to the conduction of electricity across, the lines of magnetization*. By experiments more recently made, I have ascertained that the electric conductivity of nickel is similarly influenced by magnetism, but to a greater degree, and with a curious difference from iron in the relative magnitudes of the transverse and longitudinal effects.

In these experiments the effect of transverse magnetization was first tested on a little rectangular piece of nickel 1·2 inch long, ·52 of an inch broad, and ·12 thick, being the "keeper" of the nickel horse-shoe (§ 143) belonging to the Industrial Museum of Edinburgh, and put at my disposal for experimental purposes through the kindness of Dr. George Wilson. Exactly the method described in § 175 of my previous communication referred to above, was followed, and the result, readily found on the first trial, was as stated.

The effect of longitudinal magnetization on nickel was first found with some difficulty, by an arrangement with the horse-shoe itself, and magnetizing helix (§ 143), the former furnished with suitable electrodes for a powerful current through itself, and the system treated in all respects (including cooling by streams of cold water) as described in § 156, for a correspondiug experiment on iron. The

* See Phil. Trans. Bakerian Lecture, "On the Electro-dynamic Qualities of Metals," Feb. 27, 1856, § 146 of Part 4 and Part 5. In the present communication that paper will be referred to simply by the sectional (§) numbers.

result, determined by but a very slight indication, was, as stated above, that longitudinal magnetization augmented the resistance.

The magnetization of the small piece of metal between the poles of the Ruhmkorff electro-magnet being obviously much more intense than that of the larger piece under the influence merely of the smaller helix, I recurred to the plan of experiment (§ 175) by which the effect of transverse magnetization on the little rectangular piece of nickel was first tested, and I had an equal and similar piece of iron, and another of brass, all prepared to be tested, as well as the nickel, with either longitudinal or transverse magnetic force.

To each of the little rectangles of metal to be tested, a thin slip of copper (instead of lead, as in the experiment of § 175), of the same breadth (·52 of an inch), to serve as a reference conductor, was soldered longitudinally, and to the other end of the metal tested, a piece of copper to serve as an electrode, for the principal current, was soldered. The ends of a testing conductor, 6 feet of No. 18 copper wire, were soldered respectively to the last-mentioned end of the tested metal, and to a point in the reference-conductor found, so that the resistance between it and the junction of the reference-conductor with the tested conductor, should be about equal to the resistance in the latter.

A single element, consisting of four large double cells of Daniell's (§ 63), exposing in all 10 square feet of zinc surface to 17 square feet of copper, was used to send the testing current through the conducting system thus composed, by electrodes clamped to the ends of the principal conducting channel, just outside the points of attachment of the testing conductor.

The electro-magnet was excited by various battery arrangements, in different experiments, at best by 52 cells of Daniell's, each exposing 54 square inches of zinc surface to 90 square inches of copper, and arranged in a double battery* equivalent to one battery of 26 elements each of double surface. By accident, only a single battery of 26 elements was used in obtaining the numerical results stated below.

* This arrangement was found to give about the same strength of current through the coils of the electro-magnet, as a single battery of 52 of the same cells in series, and was therefore preferred as involving only half the amount of chemical action in each cell, and consequently maintaining its effect more constantly during many successive hours of use.

The nickel was first placed between the flat poles of the electro-magnet, with its length across the lines of force, and, one galvanometer electrode being kept soldered to the junction of the nickel and the copper reference-conductor, the other galvanometer electrode was applied to the testing conductor till the point (equipotential with that point of junction) which could be touched without giving any deflection of the needle, was found. A multiplying branch, 3 feet of No. 18 wire, was then soldered with its ends $\frac{3}{8}$ths of an inch on each side of this point, and, as soon as the solderings were cool, the corresponding point on this multiplying branch was found. The magnetizing current was after that sent in either direction through the coils of the electro-magnet, and it was found that the moveable galvanometer electrode had to be shifted over about $4\frac{1}{2}$ inches on the multiplying branch towards the end of the testing conductor connected with the nickel, that is to say, in such a direction as to indicate a *diminished resistance* in the nickel. When the same operations were gone through with the nickel placed longitudinally between the poles of the electro-magnet, the zero-point on the multiplying branch was shifted about 6 inches in the direction which indicated an *increased resistance* in the nickel.

The piece of iron similarly tested, gave effects in the same direction in each case, and the results originally obtained for iron (§§ 146, 155, 161–177) were thus verified.

No effect whatever could be discovered when the piece of brass was similarly tried. It is much to be desired that experiments with highly increased power, and with a better kind of galvanometer, should be made, to discover whatever very small influence is really produced by magnetic force on the comparatively non-magnetic metals.

The shifting of the neutral point on the multiplying branch required to balance the effect produced by the longitudinal magnetization in the iron, was only from $1\frac{1}{2}$ to 2 inches. Three inches were required to balance the opposite effect of the transverse magnetization.

Hence, with the same magnetic force, the effect of longitudinal magnetization in increasing the resistance, is from three to four times as great in nickel as in iron; but the contrary effect of transverse magnetization is nearly the same in the two metals with the same

magnetic force. It may be remarked, in connexion with this comparison, that nickel was found by Faraday to lose its magnetic inductive capacity much more rapidly with elevation of temperature, and that it must consequently, as I have shown, experience a greater cooling effect with demagnetization* than iron, at the temperature of the metals in the experiment. It will be very important to test the new property for each metal at those higher temperatures at which it is very rapidly losing its magnetic property, and to test it at atmospheric temperature for cobalt, which, as Faraday discovered, actually gains magnetic inductive capacity as its temperature is raised from ordinary atmospheric temperatures, and which, consequently, must experience a heating effect with demagnetization and a cooling effect with magnetization.

The actual amount of the effects of magnetization on conductivity demonstrated by the experiments which have been described, may be estimated with some approach to accuracy from the preceding data. Thus the value of an inch on the multiplying branch would be the same as that of $\frac{1}{36} \times \frac{3}{4}$, or $\frac{1}{48}$ of an inch on the portion of the main testing conductor between its ends. The whole resistance of this $\frac{3}{4}$ of an inch of the main testing conductor, assisted by the attached multiplying branch of 36 inches, is of course less in the ratio of 48 to 49, than that of any simple $\frac{3}{4}$ of an inch of the testing conductor; but in the actual circumstances there will be no loss of accuracy in neglecting so small a difference. Hence the effect of the transverse magnetization of the nickel was to diminish its resistance in the ratio of half the length of the testing conductor diminished by $\frac{4\frac{1}{2}}{48}$ of an inch, to that of the same increased by the same, that is to say, in the ratio of $11\frac{31}{32}$ to $12\frac{1}{32}$, or of 383 to 385. Hence it appears that the resistance of the nickel, when under the transverse magnetizing force, was less by $\frac{1}{192}$, and similarly, that the resistance, when under the longitudinal magnetizing force, was greater by $\frac{1}{144}$, than when freed from magnetic influence; and that the effects of the transverse and of the longitudinal magnetizing forces on the iron were to diminish its resistance and to increase its resistance by $\frac{1}{288}$ and $\frac{1}{500}$ respectively. The first effect which I succeeded in estimating (§ 155) amounted to only $\frac{1}{3000}$, being the increase of resistance in an iron wire when longitudinally

* See Nichol's Cyclopædia of Physical Science, article 'Thermo-magnetism.'

magnetized by a not very powerfully excited helix surrounding it. In the recent experiments the magnetizing force was (we may infer) far greater.

It is to be remarked that the results now brought forward do not afford ground for a quantitative comparison between the effects of the same degree of magnetism, on the resistance to electric conduction along and across the lines of magnetization, in either one metal or the other, in consequence of the oblong form of the specimens used in the experiment. It is probable that in each metal, but especially in the nickel of which the specific inductive capacity is less than that of iron, the transverse magnetization was more intense than the longitudinal magnetization, since the poles of the electro-magnet were brought closer for the former than for the latter.

I hope before long to be able to make a strict comparison between the two effects for iron at least, if not for nickel also; and to find for each metal something of the law of variation of the conductivity with magnetizing forces of different strengths.

XX. "On the Electric Conductivity of Commercial Copper of various kinds." By Professor W. Thomson, F.R.S.

Received June 17, 1857.

In measuring the resistances of wires manufactured for submarine telegraphs, I was surprised to find differences between different specimens so great as most materially to affect their value in the electrical operations for which they are designed. It seemed at first that the process of twisting into wire-rope and covering with gutta-percha, to which some of the specimens had been subjected, must be looked to to find the explanation of these differences. After, however, a careful examination of copper-wire strands, some covered, some uncovered, some varnished with india-rubber, and some oxidized by ignition in a hot flame, it was ascertained that none of these circumstances produced any sensible influence on the whole resistance; and it was found that the wire-rope prepared for the Atlantic cable (No. 14 gauge, composed of seven No. 22 wires, and weighing altogether from 109 to 125 grains per foot) conducted about as well, on the average, as solid wire of the same mass: but, in the larger collection

of specimens which thus came to be tested, still greater differences in conducting power were discovered than any previously observed. It appeared now certain that these differences were owing to different qualities of the copper wire itself, and it became important to find how wire of the best quality could be procured. Accordingly, samples of simple No. 22 wire, and of strand spun from it, distinguished according to the manufactories from which they were supplied, were next tested, and the following results were obtained :—

Table of relative conducting qualities of single No. 22 Copper wire, supplied from manufactories A, B, C, D.

	Resistances of equal lengths.	Weights of seven feet.	Resistances reduced to equal conducting masses and lengths.	Conducting power (reciprocals of resistances) of equal and similar masses.
A. .	100	121·2 grs.	100	100
B. .	100·2	125·8 ,,	104·0	96·05
C. .	111·6	120·0 ,,	110·5	90·5
D. .	197·6	111·7 ,,	182·0	54·9

The strands spun from wire of the same manufactories showed nearly the same relative qualities, with the exception of an inversion as regards the manufactories B and D, which I have been led to believe must have arisen from an accidental change of labels before the specimens came into my hands.

Two other samples chosen at random about ten days later, out of large stocks of wire supplied from each of the same four manufactories, were tested with different instruments, and exhibited, as nearly as could be estimated, the same relative qualities. It seems, therefore, that there is some degree of constancy in the quality of wire supplied from the same manufactory, while there is vast superiority in the produce of some manufactories over that of others. It has only to be remarked, that *a submarine telegraph constructed with copper wire of the quality of the manufactory* A *of only* $\frac{1}{21}$ *of an inch in diameter, covered with gutta-percha to a diameter of a quarter of an inch, would, with the same electrical power, and the same instruments, do more telegraphic work than one constructed with copper wire of the quality* D, *of* $\frac{1}{16}$ *of an inch diameter,*

covered with gutta-percha to a diameter of a third of an inch, to show how important it is to shareholders in submarine telegraph companies that only the best copper wire should be admitted for their use. When the importance of the object is recognized, there can be little difficulty in finding how the best, or nearly the best, wire is to be uniformly obtained, seeing that all the specimens of two of the manufactories which have as yet been examined have proved to be of the best, or little short of the best quality, while those of the others have been found inferior in nearly constant proportion.

What is the cause of these differences in electrical quality is a question not only of much practical importance, but of high scientific interest. If chemical composition is to be looked to for the explanation, very slight deviations from perfect purity must be sufficient to produce great effects on the electric conductivity of copper; the following being the results of an assay by Messrs. Matthey and Johnson, made on one of the specimens of copper wire which I had found to be of low conducting power:—

Copper	99·75
Lead	·21
Iron	·03
Tin or antimony	·01
	100·00

The whole stock of wire from which the samples experimented on were taken, has been supplied by the different manufacturers as remarkably pure; and being found satisfactory in mechanical qualities, had never been suspected to present any want of uniformity as to value for telegraphic purposes, when I first discovered the difference in conductivity referred to above. That even the worst of them are superior in conducting power to some other qualities of commercial copper, although not superior to all ordinary copper wire, appears from the following set of comparisons which I have had made between specimens of the No. 22 A wire, ordinary copper wire purchased in Glasgow, fine sheet-copper used in blocks for calico-printers, and common sheet-copper.

Lengths of No. 22 A, weighing 17·3 grs. per foot, used as standards.	Conductors tested.	Their weights per foot.	Lengths resisting as much as standards if of equal conductivity.	Lengths found by experiment to resist as much as standards.	Conductivity referred to that of No. 22 A as 100.
inches.		grs.	inches.	inches.	
23·8	Ordinary No. 18 wire...............	57·5	79·0	73·6	93·2
7·5	Slip of fine sheet-copper	37·6	16·3	9·1	55·8
15·5	Slip of common sheet-copper ...	51·1	45·77	15·6	34·1

To test whether or not the mechanical quality of the metal as to hardness or temper had any influence on the electrical conducting power, the following comparison was made between a piece of soft No. 18 wire, and another piece of the same pulled out and hardened by weights applied up to breaking.

Soft No. 18 copper wire.	No. 18 copper wire, stretched to breaking.	Length found equivalent by experiment.
Weight per foot, 57·5 grs.	Weight per foot, 44·8 grs.	24·0 inches.
Length used, 30·8 inches.	Equivalent length, if of equal conductivity, 24·0 inches.	

The result shows that the greatest degree of brittleness produced by tension does not alter the conductivity of the metal by as much as one half per cent. A similar experiment showed no more sensible effect on the conductivity of copper wire to be produced by hammering it flat. There are, no doubt, slight effects on the conductivity of metals, produced by every application and by the altered condition left after the withdrawal of excessive stress*; and I have already made a partial examination of these effects in copper, iron, and platinum wires, and found them to be in all cases so minute, that the present results as to copper wire are only what was to be expected.

To find whether or not there is any sensible loss of conducting power on the whole due to the spiral forms given to the individual wires when spun into a strand, it would be well worth while to compare very carefully the resistances of single wires with those of strands spun from exactly the same stock. This I have not yet had an opportunity of doing; but the following results show that any deficiency which the strand may present when accurately compared with

* See the Bakerian Lecture, "On the Electro-dynamic Qualities of Metals," §§ 104, 105 and 150, Philosophical Transactions for 1856.

solid wire, is nothing in comparison with the differences presented by different samples chosen at random from various stocks of solid wire and strand in the process of preparation for telegraphic purposes.

No. 16 Solid Wire. Pairs of samples in different states of preparation, each 1000 inches long.

Resistances*.			Weights per foot.	Specific resistances reduced to British absolute measure.
			grs.	
Not covered....	E_1 ·2036 E_2 ·1995	·2015	74·6	11,850,000
Once covered ..	F_1 ·2054 F_2 ·1999	·2026	77·55	12,410,000
Twice covered..	G_1 ·1963 G_2 ·1963	·1963	77·2	11,970,000
Thrice covered..	H_1 ·1893 H_2 ·1916	·1904	77·73	11,680,000
	Means....	·1977	76·78	11,980,000

No. 14 Gauge Strand (seven No. 22 wires twisted together). Pairs of samples in different states of preparation, each 1000 inches long.

Resistances.			Weight per foot.	Specific resistances reduced to British absolute measure.
			grs.	
Not covered....	K_1 ·1595 K_2 ·1634	·1614	115·82	14,750,000
Once covered ..	L_1 ·1037 L_2 ·1043	·1040	109·37	8,964,000
Twice covered..	M_1 ·1426 M_2 ·1424	·1425	111·95	12,590,000
Thrice covered..	N_1 ·1092 N_2 ·1085	·1088	121·30	10,430,000
	Means....	·1297	114·61	11,680,000

* These resistances were measured, by means of a Joule's tangent galvanometer with a coil of 400 turns of fine wire, in terms of the resistance of a standard conductor as unity. The resistance of this standard has been determined for me in absolute measure through the kindness of Professor W. Weber, and has been found to be 20,055,000 German units $\left(\frac{\text{metre}}{\text{seconds}}\right)$, or 6,580,000 British units

The specific resistances of the specimens of copper wire from the manufactories A, B, C, D, of which a comparative statement is given in the first Table above, I have estimated in absolute measure by comparing each with F_2, of which the resistance in absolute measure is $6,580,000 \times \cdot 1999$, or 1,316,000. The various results reduced to specific resistances per grain of mass per foot of length are collected in the following Table, and shown in order of quality in connexion with four determinations of specific conductivity by Weber.

Specific Conductivities of specimens of Copper expressed in British Absolute Measure.

Description of Metal.	Specific resistances.
Copper wire A No. 22	7,600,000
Wire of electrolytically precipitated copper: Weber (1)	7,924,000
Copper wire B No. 22	7,940,000
Ordinary No. 18 copper wire	8,100,000
Copper wire C No. 22	8,400,000
Weber's copper wire: Weber (2)	8,778,000
No. 14 strand specimen, once covered	8,960,000
Kirchhoff's copper wire: Weber (3)	9,225,000
No. 14 strand specimen, thrice covered	10,400,000
Jacobi's copper wire: Weber (4)	10,870,000
No. 16 wire specimen, thrice covered	11,700,000
Ditto, twice covered	11,970,000
Ditto, not covered	11,850,000
Ditto, once covered	12,410,000
No. 14 strand specimen, twice covered	12,590,000
Slip of fine sheet-copper	13,600,000
Copper wire D No. 22	13,800,000
No. 14 strand specimen, not covered	14,750,000
Slip of common sheet-copper	22,300,000

$\left(\frac{\text{foot}}{\text{seconds}}\right)$. The numbers in the last column, headed "Specific resistances reduced to British measure," express the resistances of conductors composed of ten different qualities of metal, and each one foot long and weighing one grain. It is impossible to over-estimate the great practical value of this system of absolute measurement carried out by Weber into every department of electrical science, after its first introduction into the observations of terrestrial magnetism by Gauss. See "Messungen galvanischen Leitungswiderstände nach einem absoluten Maasse," Poggendorff's Annalen, March 1851. See also the author's articles entitled "On the Mechanical Theory of Electrolysis," and "Application of the Principle of Mechanical Effect to the Measurement of Electromotive Force, and of Galvanic Resistances in Absolute Units," Philosophical Magazine, December 1851.

XXI. "On the Thermal Effects of Fluids in Motion:—Temperature of a Body moving slowly through Air." By Prof. W. Thomson, F.R.S., and J. P. Joule, Esq., F.R.S. Received June 18, 1857.

The motion of air in the neighbourhood of a body moving very slowly through it, may be approximately determined by treating the problem as if air were an incompressible fluid. The ordinary hydrodynamical equations, so applied, give the velocity and the pressure of the fluid at any point; and the variations of density and temperature actually experienced by the air are approximately determined by using the approximate evaluation of the pressure thus obtained. Now, if a solid of any shape be carried uniformly through a perfect liquid*, it experiences fluid-pressure at different parts of its surface, expressed by the following formula,—

$$p=\Pi+\tfrac{1}{2}\rho\,(V^2-q^2),$$

where Π denotes the fluid-pressure at considerable distances from the solid, ρ the mass of unity of volume of the fluid, V the velocity of translation of the solid, and q the velocity of the fluid relatively to the solid, at the point of its surface in question. The effect of this pressure on the whole is, no resultant force, and only a resultant couple which vanishes in certain cases, including all in which the solid is symmetrical with reference to the direction of motion. If the surface of the body be everywhere convex, there will be an augmentation of pressure in the fore and after parts of it, and a diminution of pressure round a medium zone. There are clearly in every such case just two points of the surface of the solid, one in the fore part, and the other in the after part, at which the velocity of the fluid relatively to it is zero, and which we may call the fore and after pole respectively. The middle region round the body in which the relative velocity exceeds V, and where consequently the fluid pressure is diminished by the motion, may be called the equatorial zone; and where there is a definite middle line, or line of maximum relative volocity, this line will be called the equator.

* That is, as we shall call it for brevity, an ideal fluid, perfectly incompressible and perfectly free from mutual friction among its parts.

If the fluid be air instead of the ideal "perfect liquid," and if the motion be slow enough to admit of the approximation referred to above, there will be a heating effect on the fore and after parts of the body, and a cooling effect on the equatorial zone. If the dimensions and the thermal conductivity of the body be such that there is no sensible loss on these heating and cooling effects by conduction, the temperature maintained at any point of the surface by the air flowing against it, will be given by the equation

$$t=\Theta\left(\frac{p}{\Pi}\right)^{\frac{\cdot 41}{1\cdot 41}},$$

where Θ denotes the temperature of the air as uninfluenced by the motion, and p and Π denote the same as before*. Hence, using for p its value by the preceding equation, we have

$$t=\Theta\left\{1+\frac{\rho}{2\Pi}\left(\mathrm{V}^2-q^2\right)\right\}^{\frac{\cdot 41}{1\cdot 41}}.$$

But if H denote the length of a column of homogeneous atmosphere, of which the weight is equal to the pressure on its perpendicular section, and if g denote the dynamical measure of the force of gravity (32·2 in feet per second of velocity generated per second), we have

$$g\rho\mathrm{H}=\Pi;$$

and if we denote by α the velocity of sound in the air, which is equal to $\sqrt{1\cdot 41\times g\mathrm{H}}$, the expression for the temperature becomes

$$t=\Theta\left\{1+\frac{1\cdot 41}{2}\cdot\frac{\mathrm{V}^2-q^2}{\alpha^2}\right\}^{\frac{\cdot 41}{1\cdot 41}}.$$

According to the supposition on which our approximation depends, that the velocity of the motion is small, that is, as we now see, a small fraction of the velocity of sound, this expression becomes

$$t=\Theta\left\{1+\cdot 41\times\frac{\mathrm{V}^2-q^2}{2\alpha^2}\right\}.$$

At either the fore or after pole, or generally at every point where the velocity of the air relatively to the solid vanishes (at a re-entrant

* The temperatures are reckoned according to the absolute thermodynamic scale which we have proposed, and may, to a degree of accuracy correspondent with that of the ordinary "gaseous laws," be taken as temperature Centigrade by the air-thermometer, with 273°·7 added in each case. See the author's previous paper "On the Thermal Effects of Fluids in Motion," Part II., Philosophical Transactions, 1851, part 2. p. 353.

angle for instance, if there is such), we have $q=o$, and therefore an elevation of temperature amounting to

$$\cdot 41 \times \frac{V^2}{2a^2} \Theta.$$

If, for instance, the absolute temperature, Θ, of the air at a distance from the solid be 287° (that is 55° on the Fahr. scale), for which the velocity of sound is 1115 per second, the elevation of temperature at a pole, or at any point of no relative motion, will be, in degrees Centigrade,

$$58^{\circ}\cdot 8 \times \left(\frac{V}{a}\right)^2, \text{ or } 58^{\circ}\cdot 8 \times \left(\frac{V}{1115}\right)^2,$$

the velocity V being reckoned in feet per second. If, for instance, the velocity of the body through the air be 88 feet per second (60 miles an hour), the elevation of temperature at the points of no relative motion is ·36°, or rather more than $\frac{1}{3}$ of a degree Centigrade.

To find the greatest depression of temperature in any case, it is necessary to take the form of the body into account. If this be spherical, the absolute velocity of the fluid backwards across the equator will be half the velocity of the ball forwards; or the relative velocity (q) of the fluid across the equator will be $\frac{3}{2}$ of the velocity of the solid. Hence the depression of temperature at the equator of a sphere moving slowly through the air will be just $\frac{5}{4}$ of the elevation of temperature at each pole. It is obvious from this that a spheroid of revolution, moving in the direction of its axis, would experience at its equator a depression of temperature, greater if it be an oblate spheroid, or less if it be a prolate spheroid, than $\frac{5}{4}$ of the elevation of temperature at each pole.

It must be borne in mind, that, besides the limitation to velocities of the body small in comparison with the velocity of sound, these conclusions involve the supposition that the relative motions of the different parts of the air are unresisted by mutual friction, a supposition which is not even approximately true in most cases that can come under observation. Even in the case of a ball pendulum vibrating in air, Professor Stokes* finds that the motion is seriously influenced

* "On the Effect of the Internal Friction of Fluids on the Motion of Pendulums," read to the Cambridge Philosophical Society, Dec. 9, 1850, and published in vol. ix. part 2 of their Transactions.

by fluid friction. Hence with velocities which could give any effect sensible on even the most delicate of the ether thermometers yet made (330 divisions to a degree), it is not to be expected that anything like a complete verification or even illustration of the preceding theory, involving the assumption of no friction, can be had. It is probable that the forward polar region of heating effect will, in consequence of fluid friction, become gradually larger as the velocity is increased, until it spreads over the whole equatorial region, and does away with all cooling effects.

Our experimental inquiry has hitherto been chiefly directed to ascertain the law of the thermal effect upon a thermometer rapidly whirled in the air. We have also made some experiments on the modifying effects of resisting envelopes, and on the temperatures at different parts of the surface of a whirled globe. The whirling apparatus consisted of a wheel worked by hand, communicating rapid rotation to an axle, at the extremity of which an arm carrying the thermometer with its bulb outwards was fixed. The distance between the centre of the axle and the thermometer bulb was in all the experiments 39 inches. The thermometers made use of were filled with ether or chloroform, and had, the smaller 275, and the larger 330 divisions to the degree Centigrade. The lengths of the cylindrical bulbs were $\frac{9}{10}$ and $1\frac{4}{10}$ inch, their diameters ·26 and ·48 of an inch respectively.

Table I.—Small bulb Thermometer.

Velocity in feet per second.	Rise of temperature in divisions of the scale.	Rise divided by square of velocity.
46·9	$27\frac{1}{2}$	·0125
51·5	32	·0121
68·1	$46\frac{1}{2}$	·0100
72·7	$57\frac{1}{2}$	·0109
78·7	$67\frac{1}{2}$	·0109
84·8	74	·0103
104·5	91	·0083
130·2	151	·0089
133·2	172	·0097
145·4	191	·0090
	Mean..	·01026

The above Table shows an increase of temperature nearly proportional to the square of the velocity.

$V=\sqrt{\frac{\cdot 275}{\cdot 01026}}=163\cdot 7=$ the velocity in feet per second, which, in air of the same density, would have raised the temperature 1° Centigrade.

Table II.—Larger bulb Thermometer.

Velocity in feet per second.	Rise of temperature in divisions of scale.	Rise divided by square of velocity.
36·3	18	·0125
66·6	42	·0095
84·8	57	·0079
125·6	146	·0093
	Mean..	·0098

In this instance $V=\sqrt{\frac{330}{\cdot 0098}}=183\cdot 5$ feet per second for 1° Centigrade. It is however possible that the full thermal effect was not so completely attained in three minutes (the time occupied by each whirling) as with the smaller bulb. On the whole it did not appear to us that the experiments justified the conclusion, that an increase of the dimensions of the bulb was accompanied by an alteration of the thermal effect.

Table III.—Larger bulb Thermometer covered with five folds of writing-paper.

Velocity in feet per second.	Rise of temperature in divisions of scale.	Rise divided by square of velocity.
36·3	20	·0152
51·5	43	·0162
72·6	53	·0101
118	132	·0095

The increased thermal effect at comparatively slow velocities, exhibited in the above Table, appeared to be owing to the friction of the air against the paper surface being greater than against the polished glass surface.

One quarter of the enveloping paper was now removed, and the bulb whirled with its bared part in the rear. The results were as follow:—

TABLE IV.—Paper removed from posterior side.

Velocity in feet per second.	Rise of temperature in divisions of scale.	Rise divided by square of velocity.
75·6	60	·0105
96·8	87	·0093

On whirling in the contrary direction, so that the naked part of the bulb went first, we got,—

TABLE V.—Paper removed from anterior side.

Velocity in feet per second.	Rise of temperature in divisions of scale.	Rise divided by square of velocity.
81·7	56	·0084
93·8	72	·0082

On rotating with the bare part, posterior and anterior in turns, at the constant velocity of 90 feet per second, the mean result did not appear to indicate any decided difference of thermal effect.

Another quarter of paper was now removed from the opposite side. Then on whirling so that the bared parts were anterior and posterior, we obtained a rise of 83 divisions with a velocity of 93·8. But on turning the thermometer on its axis one quarter round, so that the bared parts were on each side, we found the somewhat smaller rise of 62 divisions for a velocity of 90·8 feet per second.

The effect of surface friction having been exhibited at slow velocities with the papered bulb, we were induced to try the effect of increasing it by wrapping iron wire round the bulb.

TABLE VI.—Larger bulb Thermometer wrapped with iron wire.

Velocity in feet per second.	Rise in divisions of scale.	Rise divided by square of velocity.
15·36	10·25	·0434
23·04	33	·0623
30·71*	49·25	·0522
46·08	68·75	·0324
69·12	98	·0206
111·34	185	·0149
126·72	207	·0129
153·55	above 280	above ·0118

* The whirring sound began at this velocity. According to its intensity the

On inspecting the above Table, it will be seen that the thermal effect produced at slow velocities was five times as great as with the bare bulb. This increase is evidently due to friction. In fact, as one layer of wire was employed, and the coils were not so close as to prevent the access of air between them, the surface must have been about four times as great as that of the uncovered bulb. At high velocities, it is probable that a cushion of air which has not time to escape past resisting obstacles makes the actual friction almost independent of variations of surface, which leave the magnitude of the body unaltered. In conformity with this observation, it will be seen that at high velocities the thermal effect was nearly reduced to the quantity observed with the uncovered bulb. Similar remarks apply to the following results obtained after wrapping round the bulb a fine spiral of thin brass wire.

Table VII.—Bulb wrapped with a spiral of fine brass wire.

Velocity in feet per second.	Rise in divisions of scale.	Rise divided by square of velocity.
7·68	2·5	·0424
15·36	13·5	·0572
23·04	36·5	·0687
30·71	48	·0509
46·08	64·5	·0304
76·8	103·5	·0175
115·18	224·5	·0169
148·78	264	·0119

The thermal effects on different sides of a sphere moving through air, have been investigated by us experimentally by whirling a thin glass globe of 3·58 inches diameter along with the smaller thermometer, the bulb of which was placed successively in three positions, viz. in front, at one side, and in the rear. In each situation it was placed as near the glass globe as possible without actually touching it.

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thermal effect must necessarily suffer diminution; unless indeed it gives rise to increased resistance.

TABLE VIII.—Smaller Thermometer whirled along with glass globe.

Velocity in feet per second.	Rise in divisions of scale.		
	Therm. in front.	Therm. at side.	Therm. in rear.
3·84	·66	10	4
7·68	2·66	40	10·5
15·36	41·9	78	51
23·04	71·2	90	71·7
38·4	78·4	90	68
57·5	99·9	112	76
70·92			107

The effects of fluid friction are strikingly evident in the above results, particularly at the slow velocities of 3 and 7 feet per second. It is clear from these, that the air, after coming in contact with the front of the globe, traverses with friction the equatorial parts, giving out an accumulating thermal effect, a part of which is carried round to the after pole. At higher velocities the effects of friction seem rapidly to diminish, so that at velocities between 23 and 38 feet per second, the mean indication of thermometers placed all round the globe would be nearly constant. Our anticipation (written before these latter experiments were made), that a complete verification of the theory propounded at the commencement was impossible with our present means, is thus completely justified.

It may be proper to observe, that in the form of experiment hitherto adopted by us, the results are probably, to a trifling extent, influenced by the vortex of air occasioned by the circular motion.

We have on several occasions noticed the effect of sudden changes in the force of wind on the temperature of a thermometer held in it. Sometimes the thermometer was observed to rise, at other times to fall, when a gust came suddenly on. When a rise occurred, it was seldom equivalent to the effect, as ascertained by the foregoing experiments, due to the increased velocity of the air. Hence we draw the conclusion, that the actual temperature of a gust of wind is lower than that of the subsequent lull. This is probably owing to the air in the latter case having had its *vis viva* converted into heat by

collision with material objects. In fact we find that in sheltered situations, such for instance as one or two inches above a wall opposite to the wind, the thermometer indicates a higher temperature than it does when exposed to the blast. The question, which is one of great interest for meteorological science, has hitherto been only partially discussed by us, and for its complete solution will require a careful estimate of the temperature of the earth's surface, of the effects of radiation, &c., and also a knowledge of the causes of gusts in different winds.

XXII. "On the Thermal Effects of Longitudinal Compression of Solids." By J. P. Joule, Esq., F.R.S.; and "On the Alterations of Temperature accompanying Changes of Pressure in Fluids." By Prof. W. Thomson, F.R.S. Received June 18, 1857.

In the further prosecution of the experiments of which an outline was given in the 'Proceedings' for January 29, 1857, the author has verified the theory of Professor Thomson, as applied to the thermal effects of laying weights on and taking them off metallic pillars and cylinders of vulcanized india-rubber. Heat is evolved by compression, and absorbed on removing the compressing force in every substance yet experimented on. In the case of metals, the results agree very closely with the formula in which e, the longitudinal expansion by heat under pressure, is considered the same as the expansion without pressure. It was observed, however, that all the experimental results were a little in excess of the theoretical, and it became therefore important to inquire whether the force of elasticity in metals is impaired by heat. In the first arrangements for this purpose, the actual expansion of the bars employed in the experiments was ascertained by a micrometric apparatus,—1st, when there was no tensile force, and 2nd, when a weight of 700 lbs. was hung to the extremity of the quarter-inch rods. The results, reliable to less than one-hundredth of their whole value, did not exhibit any notable effect of tensile force on the coefficient of expansion by heat. An experiment susceptible of greater delicacy

was now tried. Steel wire of $\frac{1}{90}$th of an inch in diameter was wound upon a rod of iron $\frac{1}{4}$ of an inch in diameter. This was heated to redness. Then, after plunging in cold water, the spiral was slipped off. The number of convolutions of the spiral was 420, and its weight 58 grains. Its length, when suspended from one end, was 6·35 inches, but on adding to the extremity a weight of 129 grains, it stretched without sensible set to 14·55 inches. The temperature of the spiral thus stretched was raised or lowered at pleasure by putting it in, or removing it out of an oven. After several experiments it was found that between the limits of temperature 84° and 280° Fahr., each degree Centigrade of rising temperature caused the spiral to lengthen as much as ·00337 of an inch, and that a contraction of equal amount took place with each degree Centigrade of descending temperature. Hence, as Mr. James Thomson has shown that the pulling out of a spiral is equivalent to twisting a wire, it follows that the force of torsion in steel wire is decreased ·00041 by each degree of temperature.

An equally decisive result was obtained with copper wire, of which an elastic spiral was formed by stretching out a piece of soft wire, and then rolling it on a rod $\frac{1}{4}$ of an inch in diameter. The spiral thus formed consisted of 235 turns of wire, $\frac{1}{40}$ of an inch in diameter, weighing altogether 230 grains. Unstretched, it measured 6·7 inches, but with a weight of 1251 grains attached to it, it stretched, without set, to 10·05 inches. Experiments made with it showed an elongation of ·00157 of an inch for each degree Centigrade of elevation of temperature, and an equal shortening on lowering the temperature. The diminution of the force of torsion was in this case ·00047 per degree Centigrade*.

* Since writing the above, I have become acquainted with M. Kupffer's researches on the influence of temperature on the elasticity of metals (*Compte-Rendu Annuel*, St. Petersburgh, 1856). He finds by his method of twisting and transverse oscillations, that the decrease of elasticity for steel and copper is ·000471 and ·000478. Very careful experiments recently made by Prof. Thomson, indicate a sl ght increase of expansibility by heat in wires placed under tension.—August 1. J. P. J.

Professor Thomson has obligingly furnished me with the following investigation :—

On the Alterations of Temperature accompanying Changes of Pressure in Fluids.

Let a mass of fluid, given at a temperature t and under a pressure p, be subjected to the following cycle of four operations in order.

(1) The fluid being protected against gain or loss of heat, let the pressure on it be increased from p to $p+\varpi$.

(2) Let heat be added, and the pressure of the fluid maintained constant at $p+\varpi$, till its temperature rises by dt.

(3) The fluid being again protected against gain or loss of heat, let its pressure be reduced from $p+\varpi$ to p.

(4) Let heat be abstracted, and the pressure maintained at p, till the temperature sinks to t again.

At the end of this cycle of operations, the fluid is again in the same physical condition as it was at the beginning, but, as is shown by the following considerations, a certain transformation of heat into work or the reverse has been effected by means of it.

In two of these four operations the fluid increases in bulk, and in the other two it contracts to an equal extent. If the pressure were uniform during them all, there would be neither gain nor loss of work; but inasmuch as the pressure is greater by ϖ during operation (2) than during operation (4), and rises during (1) by the same amount as it falls during (3), there will, on the whole, be an amount of work equal to $\varpi\, dv$, done by the fluid in expanding, over and above that which is spent on it by pressure from without while it is contracting, if dv denote a certain augmentation of volume which, when ϖ and dt are infinitely small, is infinitely nearly equal to the expansion of the fluid during operation (2), or its contraction during operation (4). Hence, considering the bulk of the fluid primitively operated on as unity, if we take

$$\frac{dv}{dt}=e,$$

to denote an average coefficient of expansion of the fluid under constant pressure of from p to $p+\varpi$, or simply its coefficient of

expansion at temperature t and pressure p, when we regard ϖ as infinitely small, we have an amount of work equal to

$$\varpi\, e\, dt$$

gained from the cycle. The case of a fluid such as water below 39°·1 Fahr., which contracts under constant pressure, with an elevation of temperature, is of course included by admitting negative values for e, and making the corresponding changes in statement.

Since the fluid is restored to its primitive physical condition at the end of the cycle, the source from which the work thus gained is drawn, must be heat, and since the operations are each perfectly reversible, Carnot's principle must hold; that is to say, if θ denote the excess of temperature of the body while taking in heat above its temperature while giving out heat, and if μ denote "Carnot's function," the work gained, per unit of heat taken in at the higher temperature, must be equal to

$$\mu\, \theta.$$

But while the fluid is giving out heat, that is to say, during operation (4), its temperature is sinking from $t+dt$ to t, and may be regarded as being on the average $t+\frac{1}{2}dt$; and while it is taking in heat, that is, during operation (2), its temperature is rising from what it was at the end of operation (1) to a temperature higher by dt, or on the average exceeds by $\frac{1}{2}dt$, the temperature at the end of operation (1). The average temperature while heat is taken in consequently exceeds the average temperature while heat is given out, by just as much as the body rises in temperature during operation (1). If, therefore, this be denoted by θ, and if $\mathrm{K}\, dt$ denote the quantity of heat taken in during operation (2), the gain of work from heat in the whole cycle of operations must be equal to $\mu\, \theta\, \mathrm{K}\, dt$, and hence we have

$$\mu\, \theta\, .\, \mathrm{K}\, dt = \varpi\, e\, dt.$$

From this we find

$$\theta = \frac{e}{\mu \mathrm{K}} \varpi,$$

where, according to the notation that has been introduced, θ is the elevation of temperature consequent on a sudden augmentation of pressure from p to $p+\varpi$; e is the coefficient of expansion of the fluid, and K its capacity for heat, under constant pressure; and μ is Carnot's function, being, according to the absolute thermodynamic

scale of temperature, simply the reciprocal of the temperature, multiplied by the mechanical equivalent of the thermal unit. If then t denote the absolute temperature, which we have shown by experiment* agrees sensibly with temperature by the air-thermometer Cent. with 274° added, and if J denote the mechanical equivalent of the thermal unit Centigrade, we have

$$\theta = \frac{t\,e}{\mathrm{JK}}\varpi.$$

This expression agrees in reality, but is somewhat more convenient in form, than that first given, Dynamical Theory of Heat, § 49, Trans. R.S.E. 1851.

Thus for water, the value of K, the thermal capacity of a cubic foot under constant pressure, is 63·447, and e varies from 0 to about $\frac{1}{2200}$, for temperatures rising from that of maximum density to 50° Cent., and the elevation of temperature produced by an augmentation of pressure amounting to n times 2117 lbs. per square foot (that is to say, to n atmospheres), is

$$\frac{t\,e \times 2117}{1390 \times 63{\cdot}447}n.$$

For mercury, we have $\dfrac{t\,e \times 2117}{1390 \times 28{\cdot}68}n.$

If, as a rough estimate, we take

$$e = \frac{t-278}{46} \times \frac{1}{2200},$$

this becomes $\dfrac{t(t-278)}{420000}n.$

If, for instance, the temperature be 300° on the absolute scale (that is, 26° of the Centig. thermometer), we have

$$\frac{n}{636}$$

as the heating effect produced by the sudden compression of water at that temperature: so that ten atmospheres of pressure would give $\frac{1}{64}$ of a degree Cent., or about five divisions on the scale of the most sensitive of the ether thermometers we have as yet had constructed.

Thus if we take $\frac{1}{5500}$ as the value of e, this becomes

$$\frac{t}{103600}n\,;$$

* See Part II. of our Paper "On the Thermal Effects of Fluids in Motion," Philosophical Transactions, 1854.

and at temperature 26° Cent., the heating effect of ten atmospheres is found to be $\frac{1}{34}$ of a degree Cent.

TABLE giving the thermal effects of a pressure of ten atmospheres on water and mercury*.

Temperature.	Increase or decrease of temperature in water.	Increase of temperature in mercury.
0°	·005 decrease	·026
3°·95	·0	·0264
10°	·006 increase	·027
20°	·015 do.	·028
30°	·022 do.	·029
40°	·029 do.	·030
50°	·035 do.	·031
60°	·041 do.	·032
70°	·047 do.	·033
80°	·055 do.	·034
90°	·065 do.	·035
100°	·078 do.	·036

XXIII. "On the Phenomenon of Relief of the Image formed on the Ground Glass of the Camera Obscura." By A. CLAUDET, Esq., F.R.S. Received June 17, 1857.

(Abstract.)

The author having observed that the image formed on the ground glass of the camera obscura appears as much in relief as the natural object when seen with the two eyes, has endeavoured to discover the cause of that phenomenon, and his experiments and researches have disclosed the singular and unexpected fact, that although only one image *seems* depicted on the ground glass, still each eye perceives a different image; that in reality there exist on the ground glass two images, the one visible only to the right eye, and the other visible only to the left eye. That the image seen by the right eye is the representation of the object refracted by the left side of the lens, and the image seen by the left eye is the representation of the object refracted by the right side of the lens. Consequently these two images presenting two different perspectives, the

* Added August 1.

result is a stereoscopic perception, as when we look through the stereoscope at two images of different perspectives.

It appears that all the different images refracted separately by every part of the lens, are each only visible on the line of their refraction when it corresponds with the optic axis, so that while we examine the image on the ground glass, if we move the head we lose the perception of all the rays which are not corresponding with the optic axes, and have only the perception of those which, according to the position of the eyes, gradually happen to coincide with the optic axes. Consequently when we look on the ground glass perfectly in the middle, the two eyes being equally distant from the centre, the right eye sees only the rays refracted from the left of the lens, and the left eye only those refracted from the right of the lens.

If we move the head horizontally, as soon as we have deviated about 6° from the centre on the right or on the left, in the first position the right eye sees no image, and the left eye sees the image which before was seen by the right eye; in the second position the inverse takes place, and of course in both cases there cannot exist any stereoscopic illusion.

When we examine on the ground glass the image of a solid produced by the whole aperture of the lens, if we have taken the focus on the nearest point of the solid, we remark, in looking with the two eyes, that the image is stereoscopic, and as soon as we shut one eye the illusion of relief disappears instantly.

The stereoscopic effect is beautifully brought out by the image of a group of trees; and when experimenting in an operating room, it is rendered quite conspicuous if we take the image of an object having several planes very distinct, such as the *focimeter*, which the author has described in a former memoir (see Phil. Mag. for June 1851).

If without altering the focus we examine the same image with the pseudoscope, the effect is pseudoscopic. But if the focus has been set on the most distant plane of the focimeter, the effect is pseudoscopic, and it becomes stereoscopic in looking with the pseudoscope.

The image loses its relief when it is produced only by the centre of the lens. The stereoscopic and pseudoscopic effects are therefore as much less apparent as the aperture of the lens has been more reduced, and they are the more evident if the image is produced by two apertures on both extremities of the horizontal diameter of the

lens. This mode of conducting the experiments presents the most decided manifestation of the whole phenomenon.

But it must be remarked, that if the image is received on a transparent paper instead of ground glass, it does not in any case present the least illusion of relief. The surface of the paper has the property of preserving to both eyes the same intensity of image from whatever direction the rays are refracted on that surface, and at whatever angle the eyes recede from the centre to examine the image. In fact, all the various images refracted through every part of the lens and coinciding on the surface of the paper, are visible at whatever angle they are examined.

The reason of this difference between the effect of the ground glass and that of the paper is, that through the surface of the ground glass, composed of innumerable molecules of the *greatest transparency*, only deprived of their original parallelism by the operation of grinding, but acting as *lenses* or *prisms* disposed at all kinds of angles, the rays refracted by the various parts of the lens continue their course in straight lines in passing through these transparent molecules, and are visible only when they coincide with the optic axes, being invisible in all other directions; that, in short, they are not stopped by the surface of the ground glass; while the paper being perfectly opaque, stops all the rays on their passage, by which the image of the object remains fixed on the surface. Each molecule of the paper becoming luminous, sends new rays in all directions; and from whatever direction we look on the paper, we always perceive at once all the images superposed, so that each eye seeing the two perspectives mingled, the process of convergence according to the horizontal distances of the same points of the various planes, cannot have its play, and no stereoscopic effect can take place, as it is the case with the ground glass, which presents to each eye an image of a different perspective.

The author explains that he has ascertained these facts by several experiments, the most decisive of which consists in placing before one of the marginal openings of the lens a blue glass, and a yellow glass before the other. The object of these coloured glasses is to give on the ground glass two images, each of the colour of the glass through which it is refracted.

The result is two images, superposed on the ground glass, one

yellow and the other blue, forming only one image of a grey tint, being the mixture of yellow and blue, when we look with the two eyes at an equal distance from the centre. But when shutting alternately, now the right eye and then the left eye, in the first case the image appears yellow, and in the second it appears blue.

If while looking with the two eyes (the opening on the right of the lens being covered with the yellow glass, and the opening on the left with the blue glass) we move the head on the right of about 6°, the mixture of the two colours disappears, and the image retains only the blue colour; on the other hand, if after having resumed the middle position, which shows again the mixture of the two colours, we move the head on the left of 6°, the mixture disappears again, and the image retains only the yellow colour.

This proves evidently that each eye sees only the rays which, when after having been refracted by any part of the lens, and continuing their course in a direct line through the ground glass, coincide with the optic axes, while all the other rays are invisible.

The consideration of these singular facts has led the author to think that it would be possible to construct a new stereoscope, in which the two eyes looking at a single image could see it in perfect relief, such a single image being composed of two images, of different perspectives superposed, one visible only to the right eye and the other to the left. This would be easily done by refracting a stereoscopic slide on a ground glass, through two semi-lenses separated enough to make the right picture of the slide coincide with the left picture at the focus of the semi-lenses. The whole arrangement may be easily understood; we have only to suppose that we look through a ground glass placed before an ordinary stereoscope at the distance of the focus of its semi-lenses, the slide being strongly lighted, and the eye seeing no other light than that of the picture on the ground glass. The whole being nothing more than a camera having had its lens cut in two parts, and the two halves sufficiently separated to produce at the focus the coincidence of the two opposite sides of the stereoscopic slide placed before the camera.

XXIV. "Supplementary Researches on the Partition of Numbers." By Arthur Cayley, Esq., F.R.S. Received March 19, 1857.

(Abstract.)

The paper is supplementary to the author's memoir, "Researches on the Partition of Numbers," which comprises the two papers abstracts of which appear in the 'Proceedings' of the Meeting of the 3rd of May, 1855. It contains some additional developments in relation to the theorem referred to at the conclusion of the former memoir, and an application to the determination of the expression for P (1, 2, 3, 4, 5, 6,) 9.

XXV. "On the Anatomy and Physiology of the *Spongiadæ*." By J. S. Bowerbank, F.R.S., F.L.S., &c. Received June 17, 1857.

(Abstract.)

The arrangement of the *Spongiadæ* by Lamarck, based entirely on external form, is wholly inadequate for the discrimination of species. The classification adopted by Drs. Fleming, Grant, and Johnston, dependent more especially on the chemical constituents of those bodies, is far too limited to be applied in generic characters. The author has, therefore, for this purpose rejected both systems, and has retained the latter one for forming primary divisions only, and he purposes founding the generic characters principally on the organic structure and mode of arrangement of the skeleton, in accordance with the practice so generally adopted by naturalists with regard to many of the higher classes of animals. *Tethea, Geodia, Dysidea* and a few others are the only well-defined genera that have yet been established; while others, such as *Halichondria*, even in the narrow circle of the list of British species, contain at least ten distinct modes of arrangement of the skeleton, each of which is constant and well-defined in its character.

It is not intended to propose the rejection of any of the well-established genera of preceding authorities, but to confine each genus strictly within the bounds indicated by the peculiar mode of structure of the skeleton which exists in that species of sponge which is

the oldest-established and best-known type of the genus, and to refer all others that may distinctly differ from that type to new genera founded on structural principles.

It is proposed to characterize the elementary tissues in the following order:—

1. Spicula.
2. Keratode or horny substance.
3. Membranous tissues.
4. Fibrous tissues.
5. Cellular tissues.
6. Sarcode.

And, in the second place, to treat of the organization and physiology in the following order:—

1. The skeleton.
2. The sarcodous system.
3. The interstitial canals.
4. The intermarginal cavities.
5. The dermal membrane.
6. The pores.
7. The oscula.
8. Inhalation and exhalation.
9. Nutrition.
10. Cilia and ciliary action.
11. Reproduction, gemmules, &c.

And to conclude with observations on the generic characters.

The author then proceeds to describe the spicula, which he states are essentially different in character from the fibres of the sponge; although the latter may be equally siliceous with the former. However closely the spicula may be brought into contact with each other, or with siliceous fibre, they appear never to unite or anastomose; while the fibre, whether siliceous or keratose, always anastomoses when it comes in contact with other parts of its own body or with those of its own species. A detailed description is given of the origin and progressive development of these organs, from which it is inferred that they are the homologues of the bones in the higher classes of animals, and that the forms they assume are always of an organic type, never crystalline or angular; and the same forms of spicula are found composed of either silex or carbonate of lime,

demonstrating the fact that the deposits of earthy matter are influenced by the laws of animal organization only, and never by those of inorganic or crystalline arrangement.

Each species of sponge has, not one form of spiculum only, equally dispersed throughout its whole substance; but, on the contrary, separate parts have their appropriate forms; and thus we find that there are often three, four, or even more forms of spicula in the same individual. The author therefore, in describing them, proposes to treat of these organs in the following order:—

1. Spicula of the skeleton.
2. Connecting spicula.
3. Defensive spicula.
4. Spicula of the membranes.
5. Spicula of the sarcode.
6. Spicula of the gemmules.

1st. The spicula of the skeleton in the siliceous sponges are usually simple, elongate in form, slightly curved, and are occasionally more or less furnished with spines. They are either irregularly matted together, collected in fasciculi, or dispersed within or upon the keratose fibres of which the skeleton is to a great extent composed. All these elongate forms of spicula are subject to extreme variety of length. In some species they maintain a great degree of uniformity, while in others they vary to a very considerable extent, according to the necessities arising from the mode of the construction of the skeleton.

2nd. The connecting spicula are not necessarily a part of the skeleton; they are a subsidiary portion of it under especial circumstances, in a few genera only, as *Geodia*, *Pachymatisma*, and other sponges which have a thick crustaceous surface, which the spicula serve to support and retain in due connexion with the mass of the animal beneath. The normal form of these spicula is very different from that of the general mass of those of the skeleton, and they are much more complex and varied in their structure. They usually have a long, stout, cylindrical or attenuating shaft terminating either acutely or hemispherically at the base, while the apex is divided into three equi-angular radii, which assume in different species a considerable amount of variety as regards form and direction. The triradiate apices are usually cemented firmly to the inner surface of the

crustular coat of the sponge; while the stout and elongated shaft is intermingled with, and firmly cemented by, keratode to the general mass of the skeleton.

3rd. The defensive spicula are divisible into two classes: those of the exterior, and those of the interior of the sponge. They are neither of them necessarily present in every species, nor are they confined to particular genera, but occur occasionally, and in certain species of various genera apparently as the necessities of the animal may render their presence requisite. Their office is evidently to defend the sponge from the attacks of predacious animals. They are projected for about half or two-thirds of their length at various angles from the surface of the sponge, or they are based on the fibre of the skeleton, and are projected at about right angles into its interstitial cavities.

4th. The spicula of the membranes are of two distinct classes. The office of the first of these is to strengthen and support those delicate tissues, and to communicate to them a certain amount of tension. The forms are few in number, and their structure comparatively simple. The office of the second class is that of assisting in the retention of the sarcode on the interstitial and other structures. They are usually minute in size, and often very complicated in form.

5th. Spicula of the sarcode. The numerous and beautiful tribe of stellate spicula appear to be devoted to connect and give substance to the gelatinoid sarcode which so abundantly covers the whole of the interior membranous structures of the sponges in which they occur. They are often exceedingly minute, and are occasionally remarkably complex and beautiful in structure, and we frequently find more than one form imbedded in the sarcode of the same sponge.

6th. The spicula appropriated to the gemmules of sponges occur in various modes of disposition. First, they are imbedded irregularly in an external envelope of the gemmule, or on the surface of the gemmule itself at right angles to lines radiating from its centre. Secondly, they are arranged symmetrically in the crust of the gemmule parallel to lines radiating from its centre. Thirdly, they are disposed in fasciculi in the substance of the gemmule from the centre to the circumference.

The forms occurring in the second class of these spicula are

exceedingly varied and beautiful, and especially characteristic of the species in which they occur.

The author has named and figured the whole of the spicula described in the paper, and has traced some of the most complicated ones from their earliest and simplest state, through all the stages of their development to the adult condition. More than a hundred distinct forms of these organs are thus described, so as to render them available hereafter to naturalists as characteristic of species.

XXVI. "Researches on the Intimate Structure of the Brain; Human and Comparative.—Part I. The Medulla oblongata." By J. Lockhart Clarke, Esq., F.R.S. Received June 18, 1857.

(Abstract.)

The medulla oblongata, as described in this memoir, extends from the first cervical nerve to the lower border of the pons Varolii. Of its elementary parts, the author first traces the *arciform fibres*, which may be divided into a superficial and a deep layer. Those of the superficial layer may in turn be divided into three sets. The connexions of these are first followed out in detail; the fibres of the deep layer are described further on. In all mammalia these fibres are very distinct, but less intricate than in man. They may be found also in birds, reptiles, and fishes.

The *anterior pyramids* are found to be composed of *four* orders of fibres:—

1. *Decussating fibres* from the *lateral columns*, forming their chief bulk.

2. *Decussating fibres* from *the posterior columns* and *posterior grey substance*, chiefly at the upper part.

3. *Decussating fibres* from the *anterior grey substance*.

4. *Non*-decussating fibres of the *anterior columns*, separate on their outer side, and on their inner side incorporated with those which form the decussation.

In mammalia generally the decussating fibres are much less numerous than in man. In birds there is an evident but feeble decussation.

Of the *corpora olivaria* it is remarked, that they are to be found not only in all mammalia, but also to a certain extent in birds. In man, the surface of each olivary body consists of two layers of fibres —transverse and longitudinal; the former in part belong to the arciform system,—the latter are continuous with the antero-lateral column. A broad transverse commissure unites the two bodies. The *corpus dentatum* is a convoluted vesicular sac, consisting of nucleated cells of small and rather uniform size, from $\frac{1}{1300}$th to $\frac{1}{1700}$th of an inch in diameter, but varying in shape, and many of them sending out processes—some one, others two, three or more. The connexion of the fibres with the convolutions of the sac is extremely complicated, and not to be made intelligible without the aid of diagrams. It may be stated, however, that the fibres which are confined to the cavity of the sac, with some others, take their origin from the cells.

In mammalia generally the olivary bodies are nearly concealed behind the pyramids, and vary in their appearance at the surface in different animals. The vesicular sac, or *corpus dentatum*, is thrown into only a few comparatively large convolutions. On the outer side of each olivary body, and separated from it by a groove which lodges the hypoglossal nerve, is another vesicular column, not hitherto described by anatomists, and of which the analogue is found in the human medulla.

As the fibres of the lateral columns cross over to the anterior pyramids, the posterior cornua sink as it were forwards, while their terminal tufts—*the gelatinous substance,* gradually increasing in bulk, reach the surface, and form the grey tubercles of Rolando. At the same time, and close to the posterior median fissure, the grey substance is raised into a small conical projection, from which a network of blood-vessels and fibres extends backwards into the posterior pyramid. Within the projection, and amongst the network, cells are developed, which are circular, pyriform, or irregular in shape, and give off one process or several. Further outwards, another, but larger projection and another network extend backwards into the restiform body, containing cells of the same character, but of superior size. These additional productions may be called respectively the *post-pyramidal* and *restiform* ganglia.

As the medulla ascends, the *root* of the *posterior cornu* and the

whole of the *anterior cornu* are gradually resolved or spread out into a beautiful and complicated network, containing a multitude of variously-shaped cells, which communicate and surround with their processes the longitudinal bundles of the white columns enclosed in the meshes. The post-pyramidal and restiform ganglia continue to increase in size, as do also the terminal tufts of the posterior cornua, which, like the former, are traversed by an extension of the network, and interspersed with cells. At the lower extremity of the olivary bodies, the decussation of the anterior pyramids, although still considerable, is very much reduced; for while its fibres derived from the lateral columns have been gradually decreasing in number, those which proceed from the posterior columns and posterior grey substance have been increasing, though not in the same proportion. These latter fibres may be traced backwards from the decussation chiefly to the restiform body and its ganglion, in which they wander in various directions, crossing each other, and becoming longitudinal.

In front, and at the side of the central canal, a new group or column of cells begins to make its appearance,—the nucleus of the hypoglossal nerve; and *behind* and on each side of the canal appears the nucleus of the spinal-accessory nerve, which is connected with its fellow of the opposite side, at the bottom of the posterior fissure, by a transverse band of fibres,—the continuation of the posterior commissure of the medulla spinalis. Of the spinal-accessory nerve, some of the upper roots proceed to their own nucleus, while others bend forwards into the hypoglossal nucleus, and in part join its fibres, to decussate through the raphè with those of the opposite nerve. The *vagus* nerve arises from a continuation of the spinal-accessory nucleus, and, on its way outwards, passes through the terminal tufts of the posterior cornu, or the so-called *gelatinous substance.* The vagal and spinal-accessory vesicular nuclei or columns, after appearing in the fourth ventricle, and diverging so as to expose the hypoglossal nucleus, sink abruptly beneath a new mass of vesicular substance which makes its appearance on each side of the ventricle. This is the auditory ganglion. It commences by a point on the outer side of the vagal nucleus, with which it is intimately connected, and is developed from the inner part of the post-pyramidal ganglion. Its cells are various in shape, and as they are developed

from the inner part of the posterior pyramid, the outer side of the latter undergoes a remarkable change of structure, and becomes the chief origin of the *anterior division* of the auditory nerve, which in its course outwards runs between the restiform body and the *gelatinous substance* or posterior cornu. The posterior division of the nerve winds transversely inwards over the restiform body to reach the auditory ganglion.

Intimately connected with the auditory nerves and ganglia is the structure which, in animals, is called the trapezium, and which the author has found to enclose a remarkable vesicular sac resembling that of the olivary bodies. The analogue of the trapezium exists in the human medulla.

The facial nerve, or portio dura of the seventh, after passing through the trapezium, proceeds transversely inwards to the *fasciculus teres*, which contains a mass or column of stellate cells, and through which it spreads, exchanging fibres or forming a loop with the sixth or *abducens* nerve, which arises from the same nucleus. The glossopharyngeal nerve has more than one origin. It passes out in several bundles through the gelatinous substance or posterior cornu, *in* which it forms a plexus with longitudinal fasciculi of fibres which may be traced upwards to the larger root of the fifth nerve.

The connecting fibres between the anterior and posterior portions of the medulla are very numerous and complicated. For convenience of description, they may be divided into two parts,—superficial and deep. The superficial are the superior layers of the arciform system, and arise from the restiform body and its ganglion: the deep fibres, more or less blended with the first, arise from the remains of the post-pyramidal and the restiform ganglia. Together they form a complicated network, or plexus, interspersed with innumerable and variously shaped cells, which frequently communicate around the longitudinal bundles of the lateral columns, through which and the olivary body the fibres of the plexus proceed forwards and inwards to the raphè, where they decussate with those of the opposite side, and become continuous with the arciform fibres which were traced to the raphè round the anterior pyramids as the superficial set. The raphè, therefore, is the seat of a very complicated

decussation between the posterior halves of the medulla, on the one hand; and on the other, between each of these and the olivary body of the opposite side.

The memoir contains the details of other observations on the medulla of man and the lower animals.

XXVII. "On the Early Stages of Inflammation." By JOSEPH LISTER, Esq., F.R.C.S. Eng. and Edin., Assistant Surgeon to the Royal Infirmary of Edinburgh. Communicated by Dr. SHARPEY, Sec. R.S. Received June 18, 1857.

(Abstract.)

In this communication the author gives an account of an investigation with which he has been recently occupied, into the process of inflammation in the Frog's foot. The paper is divided into four sections, with an introduction and conclusion.

In the introduction it is observed, that "so far from our knowledge of inflammation being in a satisfactory condition, authorities are at variance upon the fundamental question whether it is to be regarded, in accordance with John Hunter's opinion, as active in its nature, and consisting in an exaltation of the functions of the affected part, or whether it should not rather be considered a passive result of diminished functional activity......In seeking for the solution of this great problem, we cannot expect to gain much from the contemplation of the more advanced stages of inflammation......It is upon the first deviations from health that the essential character of the morbid state will be most unequivocally stamped, and it is therefore to the early stages of inflammation that our attention must be chiefly directed."

Some cases are then mentioned to show that "in the early stages of inflammation in the human subject, whether induced by mechanical irritation or by an acrid application such as mustard, or of spontaneous origin, the minute vessels become abnormally loaded with red blood, the corpuscles of which ultimately become to a greater or less extent arrested prior to the occurrence of effusion." It is afterwards shown, from numerous facts, that "conclusions arrived at from the

study of the early stages of inflammation in the foot of the frog will apply in all strictness to the same morbid process in man."

The remainder of the introduction is occupied with a sketch of the principal theories which have been proposed to account for the obstruction to the progress of the blood-corpuscles in the early stages of inflammation.

The first section of the paper is devoted to the discussion of the aggregation of the corpuscles of the blood. It is shown by the author that the *rouleaux* "are simply the result of the disk-form of the corpuscles, together with a certain, though slight degree of adhesiveness which retains them pretty firmly attached together when in the position most favourable for its operation, namely when flat surface is applied to flat surface, but otherwise allows them to slip very readily upon one another." The aggregating tendency of the red disks is thus regarded as a phenomenon similar in kind, though inferior in degree, to the well-known adhesiveness of the white corpuscles. It is further shown, from numerous experiments, that the red corpuscles vary remarkably in adhesiveness, in consequence of changes in physical circumstances, or very slight chemical action.

Section II. is on the structure and functions of the blood-vessels.

Allusion is made to a paper by the author which will shortly appear in the Transactions of the Royal Society of Edinburgh, where he has recorded the observation, that in the smallest arteries of the web of the frog's foot the middle coat is composed of muscular fibre-cells wrapped spirally round the internal membrane. The parietes of the minute arteries are thus provided with a most efficient mechanism for diminution of calibre, and contrast in this respect very strikingly with the delicate nucleated membrane which constitutes the wall of a capillary. The functions of the two sets of vessels are described as being in harmony with these differences in structure; the arteries being specially characterized by contractility, while the capillaries exhibit only such changes of calibre as are explained by elasticity.

The thinness of the capillary wall is believed to favour the mutual interchanges between the blood and the tissues, but the consideration of some facts of physiology leads the author to the conclusion, that notwithstanding the distending force of the current of blood, the liquor sanguinis is not effused as a whole among the tissues in the state of health; and this is thought to imply that there subsists a

mutual repulsion between the materials of the capillary wall and the elements of the liquor sanguinis, preventing the passage of the latter into the pores of the former, except in so far as they are attracted by the tissues for the purposes of nutrition.

The heart is believed by the author to be the sole cause of the circulation of the blood in the frog's foot, and it is proved experimentally that other sources of movement cannot have more than a very trivial influence, and that their cessation, supposing them to exist at all, does not give rise to arrest of the blood or accumulation of corpuscles in the capillaries.

Distinct evidences of muscularity and contractility have been detected in the veins of the frog's foot, but compared with the arteries, the veins show very little spontaneous contraction.

Regarding the influence of changes in arterial calibre upon the blood in the capillaries, the author is led to conclude that "the arteries regulate by their contractility the amount of blood transmitted in a given time through the capillaries, but neither full dilatation, extreme constriction, nor any intermediate state of the former is capable *per se* of inducing accumulation of corpuscles in the latter."

The influence of the nervous system upon the arteries has formed the subject of a special experimental inquiry, the results of which are given in a supplement to the paper. It is there shown that the contractions of the arteries of the frog's web are regulated by a part of the spinal cord, the irritation of which induces complete constriction of the vessels, while its destruction is followed by permanent dilatation. Neither stimulation nor removal of the nervous centre for the arteries produces any perceptible change in the quality of the blood, as respects adhesiveness of its corpuscles or otherwise.

Section III., "on the effects of irritants upon the circulation in the frog's web," commences with an account of some experiments performed with tepid water applied for a brief period to the foot. This agent, which was selected as the mildest possible stimulant, produces in a very beautiful manner constriction of the arteries, followed by dilatation, with corresponding changes in the amount of blood transmitted through the capillaries, as explained at the close of Section II. When, however, such experiments were frequently repeated upon the same animal, and especially if the temperature of the water was more elevated, effects of a different kind began to show themselves; the

corpuscles of the blood experiencing obstruction to their progress even while the arteries were fully dilated, and the vessels consequently in the state most favourable, so far as their calibre was concerned, for transmitting the current of blood. If the irritation was still continued, the minute vessels became choked with closely packed corpuscles.

Subsequent experiments with a variety of other irritating agents showed that the corpuscles, both red and white, were obstructed in their progress through the irritated part, in consequence of their tending to adhere in an abnormal degree to one another and to the walls of the vessels. The effects upon the blood were always similar, although the means employed to produce irritation were exceedingly various, such as solutions of salts, mustard, essential oils, chloroform, heat, galvanic shock, mechanical violence, &c.

The irritant was generally so applied as to act only upon a small area of one of the webs, and it was found that the abnormal adhesiveness of the blood-corpuscles was in the first instance always precisely limited to the spot which had been thus acted on, though it frequently extended afterwards more or less to surrounding parts. At the same time the vessels of the irritated spot did not differ materially in calibre from those in its vicinity which participated in the arterial dilatation induced by the stimulus. The exact correspondence between the extent of the irritant application, and that of the effect upon the blood, showed that the latter must be due to direct action either upon the blood itself or the tissues of the web. That it was not the result of direct action upon the blood was evident from the two following considerations. In the first place, most of the agents employed to cause irritation, when applied to freshly drawn blood, either had no effect upon the corpuscles, or destroyed instead of increasing their adhesiveness. Secondly, if employed so as to act mildly on the web, they induced an abnormal condition of the blood, short of actual stagnation though very apparent, namely, slow movement of numerous and adhesive corpuscles; and this state of things might last, although the time of operation of the irritant was often limited to a few seconds, or even a still briefer period. Long after all the blood which could possibly have been directly acted on had left the vessels of the part, successive fresh portions continued to experience precisely

similar changes in passing through the irritated area. Hence the author considers the conclusion to be inevitable, "that the tissues, as distinguished from changes of calibre in the blood-vessels, are the primary seat of inflammation, and that the effects on the blood are secondary results of such derangement."

The remarkable fact discovered by Dr. H. Weber of Giessen, but observed independently by the author, that accumulation of corpuscles occurs in the vessels of a part irritated, after circulation has been arrested by a tight ligature round the thigh, furnished the opportunity for careful comparison between the conditions of blood in healthy and irritated parts uncomplicated by the effects of rapid movement. A series of experiments conducted in this way confirmed the conclusion previously arrived at, that the accumulation of the blood-corpuscles was simply the result of their abnormal adhesiveness. At the same time these experiments brought out the remarkable fact, that mere quiescence of the blood does not give rise to aggregation of the red corpuscles within the vessels, unless the tissues are in an unhealthy condition in consequence of irritation. It further appeared that the corpuscles never exhibit greater adhesiveness within the vessels of an inflamed part, than do those of blood from a healthy part when drawn from the body. Also, the well-known adhesiveness of the white corpuscles within the vessels does not occur, according to the author, unless some degree of irritation is present, and never exceeds that which is always seen in blood outside the body. Hence the inference is drawn, that the tissues of a healthy part exert an influence on the blood in their vicinity, by means of which the corpuscles, both red and white, are preserved free from adhesiveness; but that in an inflamed part this influence is more or less in abeyance.

This view has been confirmed by observations made on the wing of the Bat.

Also the comparison of drops of blood from healthy and inflamed parts in the human subject showed, that so soon as the blood was withdrawn from the vessels, the corpuscles of the former presented precisely the same degree of adhesiveness as those of the latter.

At the commencement of Section IV., "on the state of the tissues in inflammation," it is stated that "the conclusion arrived at in the latter part of the last section, that blood flowing through an

inflamed part behaves itself in the same way as when separated from the living body, naturally leads us to infer that the tissues of the inflamed part are in some degree approximated to the condition of dead matter, or, in other words, have suffered a diminution of power to discharge the offices peculiar to them as components of the healthy animal frame. This inference is strongly supported by considering what common effect is likely to be produced upon the tissues of the frog's web by all the various agents known to cause inflammatory disturbance of the circulation." It is then pointed out that all these agents, though differing greatly in their nature, agree in their tendency to inflict a lesion on the part to which they are applied, and impair the functional activity of the tissues. "But strong as are the arguments thus obtained by inference, it would be very desirable to confirm them by direct observation of the tissues. It fortunately happens that the pigmentary system of the frog is a tissue which, from its peculiar form and colour, is very apparent to the eye, so that it is easy to trace the remarkably active functions with which it is endowed, and their modifications under the influence of irritation."

The author then mentions the circumstances which led him to notice that the dark pigment of the frog presents remarkable differences of appearance at different times in one and the same animal; each dark patch being sometimes of stellate figure with minutely ramifying rays, at other times in the form of a small rounded spot. These changes had been before observed by some German writers, who attributed the rounded form to contraction of the branching rays of a stellate cell. This, however, the author finds to be erroneous, and in a supplementary section "on the anatomy and physiology of the pigmentary system of the frog," shows that the cells never change in form or size, but that the pigment-granules which are suspended in a colourless fluid are capable of being, on the one hand, attracted by a central force into a small space in the body of the cell, and, on the other hand, dispersed by a repulsive power into the minutest recesses of the ramifying rays. Both concentration and diffusion of the pigment may take place with great rapidity, implying remarkable energy in the attractive and repulsive forces, both of which appear to reside in a nucleus. The supplementary section concludes with some remarks on the physiological importance of the actual

observation of such attractions and repulsions in one of the animal tissues.

The paper continues with an account of an experimental investigation into the effects of irritants upon this function of the pigmentary system. Many experiments are related, all tending to support the general proposition, that "all agents, without any exception, which have the power of inducing accumulation of corpuscles and stagnation in the blood-vessels when applied to the web, paralyse at the same time the functions of the pigment-cells." It is also shown, from experiments upon amputated limbs free from blood, that this effect is independent of the state of the circulation. In cases of slight irritation, in which the blood resumes, after awhile, its natural characters (re-solution taking place), the paralysis of the pigment-cells is only temporary. "Thus the pigmentary system of the frog is a remarkably sensitive index of the condition of the affected tissue, and it is fortunate that its physical characters render it so easy to read its pointings. . . . . The only other tissue of the frog's web, the functions of which can be observed by the eye, is that of the arterial muscular fibre-cells," and it is found that arteries passing through an inflamed area lose their power of contraction within the limits of that area, whereas the same vessels may be often seen to contract in other parts of their course.

"Thus, direct observation of the structures of the frog's web which discharge functions apparent to the eye, furnishes unequivocal support to the inference derived from other considerations, that in inflammation the tissues of the part, the primary seat of the affection, are in a state of diminished functional activity."

The "conclusion" consists of an inquiry how far the views expressed in the paper regarding the early stages of inflammation harmonize with the more advanced phenomena of the morbid process and with other facts of pathology.

XXVIII. "On the Fructification of certain Sphæriaceous Fungi." By FREDERICK CURREY, Esq. Communicated by Dr. HOOKER, F.R.S. Received May 14, 1857.

(Abstract.)

The author refers to the recent inquiries into the diversities of form existing in the reproductive organs of Fungi, and notices the physiological importance of the results, and the probable future effect upon systematic arrangement.

Two different classes of Sphæriaceous Fungi are then noticed in detail, in the former of which the different forms of fruit produced are essentially distinct, whilst in the latter the fruit is modified so as to assume a form materially different from the normal form.

The following are the plants included in the former of these classes, with the principal points noticed in each.

1. *Sphæria verrucæformis*, Ehr. The occurrence of an ascigerous and cytisporous state of fructification within the same circumscribing line, and the nature of the cells constituting that line.

2. *Sphæria favacea*, Fr. Points of distinction between it and *S. verrucæformis*. Curious modifications in the shape of the asci.

3. *Sphæria olivacea*, n. s. Aberrant forms of asci, and description of the sporidia.

4. *Sphæria tiliaginea*, n. s. The existence of spermatia and stylospores, and description of the form and modes of growth of those organs.

5. *Sphæria vestita*, Fr. The existence of perithecia and naked spores within a conceptacle common to both, and having a common orifice.

6. *Sphæria fragiformis*, Pers. Description of a secondary form of fruit belonging to the *Sphæria*, hitherto considered to have been a growth parasitical upon it.

7. *Sphæria salicina*, Pers., and *Coniothecium Amentacearum*, Corda. The production of these Fungi (hitherto supposed to be distinct plants) from the same mycelium, and the probability of a similar relation between *Sphæria lanciformis*, Fr., and *Coniothecium betulinum*, Corda.

The following are the plants included in the latter of the two classes, and the principal points noticed with regard to them.

1. *Sphæria angulata*, Fr. The occurrence of a state of fructification similar to that in the genus *Cryptosporium*; the varieties of structure in the normal sporidia, and the probable origin and nature of the abnormal fruit.

2. *Sphæria lanciformis*, Fr., and *Hendersonia polycystis*, B. & Br. Irregularity of form in the sporidia of *Sphæria lanciformis*. The growth of perithecia in the same stroma, some producing the fruit of *Sphæria lanciformis*, others that of *Hendersonia polycystis*. Notice of the probable existence of a third form of fruit of *Sphæria lanciformis*.

3. *Sphæria siparia*, B. & Br., and *Prosthemium betulinum*, Kunze. Constant association of the two forms; their external resemblance; nature of the fruit and other circumstances leading to the conclusion of the identity of the two plants.

Communications received since the end of the Session.

I. "On the Anatomy of *Tridacna*." By John Denis Macdonald, Esq., Assistant-Surgeon R.N. Communicated by Sir John Liddell, C.B., M.D., Director-General of the Medical Department of the Navy. Received June 25, 1857.

(Abstract.)

The author first explains the peculiar position which the animal of *Tridacna* occupies in its shell, in which it differs from bivalves in general. He then describes the mantle and its borders, the membranous interpallial septum, the respiratory and wide pedal openings communicating with the interpallial space, the two pairs of branchiæ, the mouth with the anterior and posterior lip and the four oral palps, the foot, the extensive cloacal cavity with its subdivisions, and the circular contractile cloacal orifice opening on the dorsal surface. He next gives an account of the form and arrangement of the ali-

mentary canal, and its relations to the liver and large ovary; and describes a large viscus situated in the space between the ovary, the adductor muscle, the base of the foot and the pericardium, divided into a central and two lateral portions, and secreting a dark brown liquid loaded with fatty matter. This body he thinks may be connected with the secretion of the byssus, but, at the same time, remarks that it may be homologous with the organ of Bojanus. Lastly, the anatomy of the heart and great arteries is given, and is in substance as follows.

On cutting through the floor of the cloaca, the pericardium is laid open, and in it is seen the large, rather square-shaped ventricle, with a capacious but thin-walled auricle opening into it on either side, through an orifice guarded by semilunar valves. From the thick-walled ventricle, a short tube conducts into a conical dilatation or *bulbus arteriosus*, with muscular walls, having its base included in the pericardium, and giving rise near its narrow end to the anterior and posterior pallial arteries; whilst a visceral artery passes from the ventricle to the ovary and adjacent parts. As in other bivalves, the intestine, before its termination, passes through the heart: in coming through the pericardium, surrounded by that membrane, it forms a short round pedicle which joins the fore part of the ventricle; it is then continued through the ventricle and bulbus arteriosus, and finally opens into the cloaca. The blood from the ventricle flows between the outer surface of the intestine and the inside of the sanguiferous channel; and "that part of the intestine which traverses the bulbus arteriosus is closely surrounded with elongated membranous valvulæ, which arise from the anterior part of the chamber where the gut enters, and are fixed by a number of cordæ tendineæ to the posterior wall, where it makes its exit;" a contrivance which permits the blood to pass between the rectum and the little valves, but prevents its reflux.

The description is illustrated by drawings of the exterior of the animal and of the particular parts specially referred to, and also by a diagram representing the general arrangement and relative position of the several organs.

II. "Summary of a paper (to be presented) entitled Experimental Researches on the Spinal Cord as a leader for Sensibility and Voluntary Movements." By E. BROWN-SÉQUARD, M.D. Communicated by JAMES PAGET, Esq., F.R.S. Received June 25, 1857.

The new field opened by the genius of Sir Charles Bell is enlarging every day, and interesting advances in Physiology and Pathology are constantly being made which are due to the admirable discoveries of this eminent biologist. The following results of my experiments are new developments of these discoveries.

I. It has been well proved by the researches, of which I have already published the results, that the nerve-fibres employed to convey sensitive impressions, may be deprived of sensibility, so that the property of being sensitive and that of conveying sensitive impressions are distinct one from the other. A similar distinction must be made as regards the voluntary motor nerve-fibres. I have found them inexcitable by some of our means of excitation in places where there are a great many of them conveying the orders of the will to muscles. When we introduce a needle gently through the medulla oblongata from behind forwards, traversing successively one of the restiform bodies, the descending root of the trigeminal nerve, some gray matter and the anterior pyramid, not only the animal does not give signs of pain, but there is usually no movement produced in any part of the body. The precautions which must be taken to obtain this result are described in the paper; I will merely say here that if the needle is introduced obliquely instead of perpendicularly, a spasm of the muscles of the neck occurs, and sometimes the animal rotates.

This experiment shows that the irritation of the anterior pyramids with a needle, does not excite more movement than that of the cerebral lobes, although these pyramids are certainly channels for the transmission of orders of the will to muscles. It results from this fact, that the conclusion, drawn by some physiologists, from the inexcitability of the cerebral lobes, that the voluntary motor nerve-fibres do not go into them, is not well-grounded.

I have found that the olivary tract of the medulla oblongata is both sensitive and motor, although it is not, as I have tried to prove elsewhere, a leader for sensibility and voluntary movements.

II. Some experiments which I made two or three years ago, and many that I have recently performed, show that the lateral columns of the spinal cord in the neighbourhood of the medulla oblongata, and a part of the anterior gray matter, are the principal, if not the only channels for voluntary movements in that region. Lower down in the spinal cord (in the dorsal and lumbar regions), the anterior columns and the gray matter seem to have alone the function of conductors for voluntary movements. If a transverse section be made of one of the lateral columns with almost the whole of the anterior horn of gray matter, about an inch behind the medulla oblongata, we find voluntary movements almost completely lost on that side. If a transverse section be made of one of the anterior columns of the spinal cord, an inch behind the medulla oblongata, we find that voluntary movements are not much diminished; and, as in this case, we divide also a part of the gray matter, and frequently a part also of the lateral column, it seems that the anterior column there has but little to do with voluntary movements. Other experiments lead to the same conclusion; I will relate the following alone: the anterior columns of the cord, when arrived at the medulla oblongata, are known to send most of their fibres into the lateral parts of this nervous centre, where they may be divided easily. I have found, after the transverse section of the lateral column of the medulla oblongata, that the voluntary movements are very little diminished, and sometimes hardly at all.

Besides, if we divide longitudinally the medulla oblongata, where the pyramids cross each other, we find that the voluntary movements are completely lost in both sides of the body. There are convulsive and reflex movements, but no spontaneous regular movement, and the animals, when put on their feet, fall on one side or the other.

From these facts and many others, it seems:

1st. That the anterior pyramids of the medulla oblongata contain most of the voluntary motor nerve-fibres.

2nd. That in the cervical region of the spinal cord, the voluntary

motor nerve-fibres are mostly in the lateral columns and the anterior gray cornua.

3rd. That, in the dorsal and lumbar regions of the spinal cord, these nerve-fibres are in the anterior columns and in the gray matter.

III. I have shown elsewhere that the transmission of sensitive impressions continues to take place after a transverse section of either the posterior, the lateral, or the anterior columns of the spinal cord; I have shown, also, that after a section of these three columns on the two sides, leaving the central gray matter and a great part of the posterior and anterior gray horns as little injured as possible, the transmission of sensitive impressions continues to take place, although diminished.

Since the publication of my researches on this subject, I have found that the anterior columns contribute positively, though but very little, to the transmission of sensitive impressions. In frogs, in birds, and also in higher animals, I have found that after a complete transverse section of the whole of the spinal cord, except the anterior columns, there are traces of sensibility in the parts of the body which are behind the section. During half an hour, sometimes much longer, after the operation, there is no appearance of sensibility, but after a time sensibility becomes evident. It is not to be found everywhere behind the section, but it exists in many parts of the skin. I was for a long time doubtful on this subject, because many times, in examining carefully the section of the spinal cord, after this organ had become very hard from having been immersed several days in alcohol, I have seen that a small quantity of gray matter had been left undivided at the bottom of the wound. But in multiplying the experiments, I have ascertained beyond all doubt that when the whole of the gray matter has been divided, with even some fibres of the posterior surface of the anterior columns, traces of sensibility could after a certain time be found.

It has of course been impossible to divide absolutely the whole of the gray matter, without dividing at the same time a greater or smaller number of the anterior columns. In looking at the result of this experiment, and comparing it with that of various others, in some of which very little of the gray matter had been left, while in others the number of the divided fibres of the anterior columns was greater than usual, I have found that in the anterior columns,

only a thin layer of fibres seems to be employed in the transmission of sensitive impressions, this layer forming the surface of these columns which is in contact with the gray matter.

IV. After having ascertained that there are some nerve-fibres in the white anterior columns which are employed in the transmission of sensitive impressions, I have tried to find if these fibres are sensitive or not, that is, able to give pain as the posterior roots of nerves, or unable to give pain, as the gray matter; and have found that they seem to be totally deprived of sensibility.

III. "Summary of a paper (to be presented) on the resemblance between the effects of the section of the Sympathetic Nerve in the Neck and of a transverse section of a lateral half of the Spinal Cord." By E. Brown-Séquard, M.D. Communicated by James Paget, Esq., F.R.S. Received June 25, 1857.

I will merely indicate here the principal points of similitude between the effects of these two experiments. Some of the results here mentioned as observed after the section of the sympathetic nerve in the neck, have been discovered by Prof. Bernard; the others have been found by myself. As to the effects of the section of a lateral half of the spinal-cord, I have discovered all of them.

Section of the cervical sympathetic nerve; its effects on the corresponding side of the face.	*Section of a lateral half of the spinal cord in the dorsal region; its effects on the posterior limb, on the corresponding side.*
1. Blood-vessels dilated (paralysed).	1. The same effect.
2. As a consequence, more blood.	2. Ditto, ditto.
3. Elevation of temperature.	3. Ditto, ditto.
4. Sensibility slightly increased.	4. Very much increased.
5. Ditto lasting longer there than on the other side, when the animal is chloroformized.	5. Lasting longer than anywhere else during chloroformization.
6. Sensibility lasting longer there than on the other side during agony.	6. Lasting longer than anywhere else during agony.
7. Many muscles contracted.	7. A state of slight contraction in many muscles.

8. Absorption more rapid.	8. The same effect.
9. Increase of sweat and other secretions.	9. Increase of sweat.
10. Reflex movements last longer there than elsewhere, after death.	10. The same effect.
11. After poisoning by strychnia, the first convulsions take place.	11. The same effect.
12. A galvanic current too weak to excite contraction elsewhere, may act there.	12. The same effect.
13. The motor nerves, after death, remain longer excitable there than on the other side.	13. The motor nerves, after death, reremain *notably longer* excitable there.
14. The muscles, after death, remain longer contractile there than on the other side.	14. The muscles, after death, remain *much longer* contractile there.
15. The contractility of blood-vessels is greater and lasts longer there.	15. The same effect.
16. The galvanic muscular current (as ascertained with the rheoscopic frog) is stronger and lasts longer there than on the other side.	16. The same effect (more marked).
17. Cadaveric rigidity appears later there than on the other side, and it lasts longer.	17. Cadaveric rigidity appears *notably later* there than elsewhere and lasts longer.
18. It is easier to regenerate there than on the other side the vital properties of nerves and muscles by injections of red blood, a short time after they have disappeared.	18. The same effect (more marked).
19. Putrefaction comes on later, and seems to progress more slowly there than on the other side.	19. The same effect (more marked).

All these effects, in the two cases, seem to be produced in the following manner:—There is a paralysis of the vascular nerves and therefore a paralysis of the blood-vessels; in consequence of this paralysis, the blood arrives in larger quantity and therefore the temperature is higher; nutrition is increased, and, in consequence, the vital properties of nerves, muscles and blood-vessels are augmented. Other causes contribute to the increase of the vital properties of nerves and muscles in the posterior limb after section of a lateral half of the spinal cord; among them I may point out the influence of the

oxygen of the atmosphere on the spinal cord, the paralysis of the blood-vessels of this organ, and the state of rest of the muscles and motor nerves of the limb.

If we compare the side of the face where the sympathetic has not been divided, with the posterior limb on the uninjured side of the spinal cord, we find that they also have a great many points of resemblance. They both receive less blood than usual, their temperature diminishes, their nutrition is less active, and their vital properties also diminish.

IV. "Experimental Researches on the Influence of Efforts of Inspiration on the Movements of the Heart." By E. Brown-Séquard, M.D. Communicated by James Paget, Esq., F.R.S. Received June 25, 1857.

A very interesting fact, of which many circumstances have been carefully investigated by Professor Donders and Dr. S. W. Mitchell, has received a wrong explanation from those physiologists. This fact consists in a diminution of either the strength or the frequency of the beatings of the heart, when an energetic effort at breathing is made and maintained for half a minute or a little more. Professor Donders thinks that this influence of inspiration on the heart is due to a mechanical agency of the dilated lungs on this organ.

They admit that the state of the lungs has a great influence on the heart, but the principal cause of the diminution in the movements of this organ is very different from what has been supposed by Professor Donders, by Professor J. Mueller, and others. It is known that when the medulla oblongata, or the par vagum are excited (either by galvanism, as the Brothers Weber have discovered, or by other means, such as a mere compression, or a sudden wound, as I have found), the heart's beatings diminish or cease entirely. Whether this stoppage be due to the cause I have attributed it to or not, is indifferent to my present object. What is important is, that in these cases an irritation on the origin of the par vagum acts through it on the heart to diminish or to destroy its action.

I thought that it would be interesting to decide, if, at the time that there is an effort at inspiration, there is not also an influence of the medulla oblongata on the par vagum, more or less similar to that which exists when we galvanize or otherwise irritate the medulla oblongata. To ascertain if it is so, I have made experiments on newly-born animals, and on birds. As I have already published some of the results of my researches on newly-born animals, and as these results are not so completely decisive as those of my experiments made on birds, I will merely give here a summary of what I have seen in these last animals. I have found the same facts in ducks, geese and pigeons, but as I have repeated the experiments more frequently on the last-mentioned animals, I will speak of them only. When their abdomen has been widely opened and their heart exposed to sight, pigeons may live, as it is well known, for a long while. I wait until they are almost dying, having only one, two, or three inspirations in a minute, and then, if the weather is cold, and if the animal has lost many degrees of its temperature, I find that, at each effort it makes to inspire, the heart either almost suddenly stops, or beats much less quickly.

I have frequently seen the heart completely arrested for five or ten seconds, and twice for twenty or twenty-five seconds, in cases where there was only one respiration in two minutes. This stoppage of the heart's movements was the more remarkable, as they were at the rate of more than two hundred in a minute, when the effort at inspiration took place. To decide that it was in consequence of an influence of the par vagum that this occurred, I divided this nerve in the neck, and then found that there was no more influence of the inspiration on the heart, or if there was, it consisted in an augmentation of the frequency of the movements of this organ—an augmentation due to the shaking of the heart when the chest dilated.

Sometimes, when the heart was very irritable, and when the efforts at inspiration were still frequent and not energetic (the par vagum being undivided), these efforts were accompanied, or rather immediately followed, by an increase in the strength of the heart's movements, probably caused by the shaking. But always when the inspiratory efforts were energetic and rare, they co-existed with a diminution or a momentary cessation of the heart's contractions; and always in these cases the section of the par vagum has destroyed

the diminishing influence of the respiratory efforts on the heart. It would be easy to show that the influence of the inspiratory effort on the central organ of circulation is comparable to the change taking place in the pupil when the globe of the eye is drawn inwards: it is an associated action.

From the facts I have found in the case of newly-born animals and birds, and from the facts observed in man by Professors J. Mueller, Donders, and others, it results that, during efforts at inspiration, a nervous influence passes along the par vagum from the medulla oblongata to the heart, diminishing the movements of this organ. And as by an action of our will we may inspire with energy, it follows that we can by an influence of our will diminish the action of our heart, just as we can contract our pupil by drawing our eyes inwards.

V. "Summary of a Paper (to be presented) on the Influence of Oxygen on the vital properties of the Spinal Cord, Nerves, and Muscles." By E. Brown-Séquard, M.D. Communicated by James Paget, Esq., F.R.S. Received June 25, 1857.

The influence of oxygen and carbonic acid on the living tissues, has been very little investigated, either by physiologists or practitioners of medicine. I have made a great many experiments on this subject, but will relate here merely a few of them, which are sufficient to show that oxidation of the spinal cord and nerves, as well as that of muscles, increases their vital properties, sometimes in a high degree.

After the opening of the spinal canal, the dura-mater being laid bare, we find that an evident hyperæsthesia appears after a short time in the parts of the body which are behind the opening, and also on the same level with it, and a little above it. I think this increase of sensibility depends on the absorption of oxygen. To ascertain that it is so, with the help of a special apparatus, immediately after laying bare the cord, I pump out the air in contact with the dura-mater, and substitute for it hydrogen. Then I find

that no increase of sensibility takes place, at least during several hours. I then pump out the hydrogen, and inject atmospheric air, and in a few minutes the posterior parts of the body become hyperæsthetic.

When the posterior columns of the spinal cord have been divided transversely, there is, as I found long ago, an excessive hyperæsthesia in all the parts of the body situated behind the section, and also in some of the parts immediately above it. This hyperæsthesia begins almost at once after the operation, and increases for many hours, and sometimes for one or two days. It diminishes a little afterwards, and if there is no myelitis, it continues to exist, though less than at first, for years after the operation. A part of this hyperæsthesia (its great excess during the first hours and days), I have recently found to depend chiefly upon the influence of the oxygen of the air. If we perform the section of the posterior columns of the spinal cord, and cover the wound immediately afterwards with the apparatus above-mentioned, and pump out the air and inject hydrogen, we find that there is a delay in the development of hyperæsthesia, and that it is never as considerable as when air is in contact with the injured cord. If we replace the hydrogen by air, there is a rapid increase of the hyperæsthesia, and there is still more so if we inject pure oxygen. When the hyperæsthesia has once become excessive, hydrogen does not diminish it.

If carbonic acid is injected, slight convulsive movements are produced, and sensibility soon diminishes.

Analogous experiments on sensitive and motor nerves show that in them also an increase of the vital properties is produced by oxygen, and that a diminution after an excitation is produced by carbonic acid. Experiments on the abdominal sympathetic give similar results.

Rolando and others have found that the gray matter which is in the rhomboidal ventricle of the lumbar enlargement of the spinal cord in birds, is not excitable either to give pain or produce movement. I have ascertained that normally it is but little excitable, but that when it has been exposed to the contact of air for some 10 or 15 minutes, it is extremely excitable, particularly for the production of movements. This, perhaps, explains the curious disturbance in the voluntary movements which I found some years ago in birds, on which this gray matter had been simply laid bare.

From these facts, and a great many others, it results that in the spinal cord, the sensitive and motor nerves, and the sympathetic, as well as in muscles, there is an increase of the vital properties produced by oxygen.

VI. "Summary of a paper (to be presented) on the Power possessed by Motor and Sensitive Nerves of retaining their Vital Properties longer than Muscles, when deprived of Blood." By E. Brown-Séquard, M.D. Communicated by James Paget, Esq., F.R.S. Received July 3, 1857.

It is an admitted doctrine that the vital properties of motor and sensitive nerves disappear much sooner than those of muscles, after death, or when they are deprived of blood. Although founded on positive facts, this theory is not correct, these facts being capable of another interpretation. In the experiments made heretofore, other organs (such as the spinal cord and muscles) had been deprived of blood, at the same time with the sensitive and motor nerves. To ascertain the influence of the deprivation of blood on nerves alone, it was necessary to experiment otherwise than has been done hitherto. I will in this short summary show, only by a few experiments taken at random among many, that the essential vital properties of sensitive and motor nerves can last longer than muscular irritability after a complete or almost complete deprivation of blood.

Exp. 1.—After having placed ligatures around the arteries of the upper part of the thigh of a young Guinea-pig, I amputated the limb, leaving the sciatic and crural nerves uninjured. In the whole length of the thigh I then laid bare these nerves, and dissected their neurilemma all along, so as to cut away as much as possible the little supply of blood they received from above. Voluntary movements existed about 8 minutes in the amputated leg; excitation of the trunks of the exposed nerves continued to produce muscular contractions for 6 minutes more.

In the whole length of the amputated limb, 10 minutes after the operation, sensibility was greater than in the other limbs. Muscular irritability disappeared in the amputated leg from 30 to 40

minutes after the operation, and cadaveric rigidity began in some of the muscles between the 32nd and 35th minute, and was fully established between the 45th and 50th minute.

For more than 70 minutes sensibility remained in the toes and in the skin over the rigid muscles. There was a slight degree of sensibility still evident in the skin of the knee 110 minutes after the operation.

It follows therefore that the duration of the essential vital property of the sensitive nerves may last at least twice as long as the essential vital properties of muscles after deprivation of blood.

Exp. 2.—In a large adult Rabbit I divided the posterior columns of the spinal cord in the dorsal region, so as to leave the sensibility of the hind limbs increased. I then amputated one of these limbs in the upper part of the thigh, leaving the sciatic and crural nerves uninjured, except that I took away their neurilemma in the whole length of the thigh.

The amputation had been made at nine o'clock, A.M.

At 9·15 there was a notable hyperæsthesia in the two hind legs, greater in the amputated side than in the other.

At 10·20′ the hyperæsthesia was at least as great in the amputated limb as in the other.

At 11·15 muscular irritability was much diminished in the amputated limb; sensibility was still very great in this limb, but inferior to that of the other.

At 12 M. no trace of muscular irritability existed in the amputated limb, and cadaveric rigidity had begun almost everywhere. Sensibility, though diminished, was still as great as in the anterior limbs.

At 1 P.M. traces of sensibility were still very evident in the whole skin of the amputated limb.

At 1·15 still some traces of sensibility, which disappeared at 1·30.

In this experiment, as well as in the preceding, the sensitive nerves kept their vital property much longer than the muscles after deprivation of blood.

In all the similar experiments that I have made, I have obtained the same result. But as I have found and shown elsewhere that the vital properties of nerves may be increased by the influence of the oxygen of the atmosphere, there was in the experiments above related a cause of error in the comparison of muscles with sensitive nerves.

Besides, in one of the experiments I have mentioned, the sensibility had been increased in consequence of the section of the pósterior columns of the spinal cord. I will therefore relate an experiment in which there was no cause of increase of the vital properties of the sensitive nerves.

Exp. 3.—On a vigorous adult male Guinea-pig I tied the common iliac artery, on the two sides, at 15 minutes past three, P.M.

At 3·25 very weak voluntary movements remained in the posterior limbs; their sensibility almost normal.

From 3·25 to 3·35 slight convulsions in the posterior limbs.

At 4 o'clock muscular irritability was much diminished in the posterior limbs; sensibility not so much diminished.

At 4·30 cadaveric rigidity had begun everywhere, in the posterior limbs; sensibility, though greatly diminished, still existed.

At 5·20 cadaveric rigidity very strong; traces of sensibility still very evident.

At 5·50 the last traces of sensibility disappeared.

This experiment shows that the vital property of sensitive nerves, deprived of blood, lasts longer than that of muscles in the same circumstances.

In the experiments made by other physiologists on this subject, they had put a ligature around the aorta, sufficiently high to diminish circulation in the lumbar region of the spinal cord, and the loss of sensibility which then soon took place in the posterior limbs depended upon the diminution of function of the spinal cord, and, as my experiments show, not on the loss of the vital property of the sensitive nerves of the posterior limbs.

To find out the difference of duration of the vital property of motor nerves and of that of muscles, I have made many experiments of the following kind:—

Exp. 4.—I laid bare the sciatic nerve in the whole length of the thigh of a strong adult Rabbit, and dissected its neurilemma, so as to cut away all the small blood-vessels running on this nerve. I then did the same thing with the crural nerve. Three hours afterwards, a slight diminution of voluntary movement and of sensibility was observed; two hours later, sensibility and voluntary movement were still persisting, though notably diminished.

Five hours still later, sensibility had increased, while voluntary

movement remained the same. I then divided the two nerves as high as possible, and, afraid that there might be some small blood-vessels still giving blood to the nerves, I dissected the whole length of their trunk for the second time. *Near the section, the nerves remained able to cause muscular contractions for seven hours.*

Experiments more or less similar to this one have given very nearly the same results, and I am therefore led to conclude that, with the least quantity of blood, motor nerves retain their vital property *very much longer* than muscles. If motor nerves in a limb separated from the body of a living animal seem to lose their vital property sooner than muscles, it is because, as I will prove in another paper, the transmission of the nervous force from the last nervous ramifications to the contractile elements of muscles, soon becomes impossible in the absence of blood charged with oxygen.

From the facts above related, and from many others, I think I am entitled to conclude that the vital properties of motor and sensitive nerves may last longer without blood than muscular irritability.

VII. "Ocular Spectres, Structures and Functions, Mutual Exponents." By James Jago, A.B. Cantab., M.B. Oxon., Physician to the Royal Cornwall Infirmary. Communicated by R. Were Fox, Esq., F.R.S. Received August 22, 1857.

(Abstract.)

Section I.—*Introduction.*

Our visual organs are not only capable, by an adjusting lenticular system, of painting, under varying conditions, images of luminous objects, upon a membrane in special relation with the brain, but involve many adjuvant structures; and thus it happens that they reveal to us a number of adventitious phenomena—spectres as we may call them, whether caused by light at the parts that cover the eyeballs, or within them, or by any stimulus whatever affecting the special nervous tract. These must be eliminated, if we would avoid the risk of ascribing effects begotten by subordinate parts to more integral portions of the apparatus.

Finally, they may be rendered serviceable for the solution of certain important points of ocular structure and function. Under the impression that so diversified a subject has not yet received all the elucidation of which it is susceptible, another *methodical* attempt to investigate it will be made in this memoir.

When light is an agent in the production of the spectral phenomena, they arise from certain rays being blocked from their course, at some obstacle they encounter, or turned aside by refraction, reflection or inflection. And to make *precise* observations upon them, we must use fine pencils of rays which do not return to foci upon the retina. In order to estimate the relative and actual sizes, localities, and characters, of corpuscles whose shadows or images are projected upon the retina, pencils of light which are first convergent, and therefrom, by passing through foci, divergent (such as may be conveniently obtained from a small disc of light at a sufficient distance from the eye, when viewed through a lens of an inch focal length), are mainly employed, the foci being carried from before the eye to various depths in its interior *. If we neglect ocular refractions, whether a body fall in the convergent or divergent portions, the length of its shadow will be to its own, as their respective distances from the focus. With a couple of such pencils, whether a body fall in the convergent or divergent portions, the distance between its pair of shadows is to that between the foci, as that of the object from the shadow screen (retina) to that of the object from the line (parallel

* In the Allgem. Encyklop. der Physik, s. 166 (1856), in an able article on "Entoptics," Helmholtz states, that "the more decided entoptical 'methods' were established first by Listing and Brewster (1845), who were followed still later by Donders (1846–51)." The present writer refers to a paper of his own, published early in 1845, which substantially gives the methods alluded to; which are all modifications of one idea, that of obtaining a greater parallactic deviation of shadows for objects further from the retina, by means of two pencils of rays diverging from points in front of the eye, or by one moved across the optic axis. Now we can by this device only get marked differences in parallax for small differences in ocular depth for objects very near to the points of divergence, that is, near the surface of the eye; whereas the plan now proposed not only generally secures this end in a notable degree, but by placing foci between any two objects, causes their shadows to be deflected in *opposite* directions, and the more considerably as they are nearer together; besides supplying, it is believed, a variety of aids in entoptical researches.

to the screen) which joins the foci. But the deflections of the shadow in the two sorts of rays are in *adverse* directions, so that if the axis of a single pencil were moved across the eye, whilst always kept parallel to itself, the shadows of all the objects lying in advance of the focus would travel in one direction, whilst those of objects lying behind the focus would travel in the opposite, the rate of movement being in both instances greater for objects nearer the focus. Also, whether with a couple of pencils or a single one in movement, for a given difference in ocular depth between two objects, the difference in parallactic deviation is greater as the two objects are nearer the foci or focus. Besides, generally, the picture of the contents of the eye, as shown by the convergent portion, is inverted in the divergent.

The above proportions are turned into equations which indicate, as the terms alter their values, every observed variation in the sizes of the shadows, parallactic deviations, inversions of figure, place, and movements whether of the ocular bodies with respect to the pencils, or of the foci and axes of the pencils with respect to them. Thus, in many modes, which are explained, we may at pleasure by mere inspection observe the structural and relative positions of the bodies in question, and can so manage to evade the effects of ocular refractions, as to render the proportions above stated available for calculating the sizes of the bodies, and their distances from the cornea, iris, faces of the crystalline lens, retina, or from any one of themselves —or can even measure their depths in the eye, almost without calculation, and free from any that involve the consideration of the optical qualities of the organ.

Inflective phenomena are alike in the convergent and divergent rays, but refractive differ, and afford us a useful means of detecting the nature of an object. Inflective coloration is too subordinate to the ocular chromatic dispersion to deserve particular notice. Dr. T. Young explains how narrow straight objects, viewed through a puncture, are by the influence of ocular refractions made to appear curved, unless they are seen as diameters of the projected image of the pupillary opening. It is appended, that if they are made to encroach, in a like way, laterally upon a divergent pencil, they appear not, as in the case mentioned, concave, but convex towards the centre of the pupil's image.

Diagrams and other drawings accompany these and other parts of the essay. The foregoing principles are henceforth applied to the actual exploration contemplated, in order, as follows:—

Section II.—*Apparitions from Eyelashes, Eyelids and Conjunctival Fluids.*

These phenomena are treated with an effort at greater precision than in previously existing accounts of them. Little bars of fluid along the margins of the lids are shown to occasion the long beams of light, which issue from flames regarded "with winking eyes," by their annulling the refractions of the cornea. These beams have been ascribed to reflection at the edges of the lids, but reflection only yields a very pale beam which can be distinguished easily from the other.

Section III.—*Apparitions from Iris and Crystalline Lens: with Corollaries.*

The margin of the iris, opaque and transparent bodies, and the structural stellate figure, in the crystalline lens are placed *methodically* in the order in which they lie in the depths of the eye, and the especial manifestations which they severally yield, explained. The combined effects of ocular chromatic aberration, inflection at the edge of the iris, and the limbs of the stellate figure when we look at thin objects, or black and white lines, especially if curved, render some singular illusions, which are dissected.

The method by the two sorts of pencils may be applied to test the recent doctrine advanced by Stellweg, that the iris so lies on the face of the crystalline lens, that there is no posterior chamber in the aqueous humour, and will probably be found to disprove it.

A calculation is entered into to show that unless Dr. T. Young—in estimating that the accommodation of the eye to focal distance by means of an alteration in the length of the optic diameter, would require a faculty of doing this to the extent of $\frac{1}{7}$th of the whole, taken when vision is suited to parallel rays,—exceeds the truth by *many times*; it must be easy to detect, by the parallax of the lenticular corpuscles in a couple of pencils whose foci rest near them, how and where the change is effected. And then an argument is drawn, that

the accommodation is by change in the form of the lens, producing a minute movement of its anterior face, which it is thought may be detected by the said method.

The want of symmetry in ocular refractions is glanced at, and a nebulous scattering of light in the eye,—hereafter found to be the cause of a singular supplementary version of Purkinje's vascular phantom.

Section IV.—*Apparitions from the Vitreous Humour, applied to explain its Structure.*

It can be observed that, *in the posterior chamber of the eye there exists a lax, irregular, fibrous network, springing from the hyaloid membrane, but spanning the crystalline lens, without attachment to its capsule, occupying principally the peripheral portion of the cavity, but spreading as one structure into its interior, towards an ever-lessening number of leading fibres. The whole system is of less specific gravity than the vitreous fluid, either of itself, or by being the framework of membrane, in more or less of its extent. The fibres are constituted entirely of rows of beads, which are round, or nearly so, transparent, and of greater refractive power than the fluid, and joined by passing into one another by small portions of their surface.* The dynamical and optical considerations upon which these conclusions depend, are very carefully entered into, and the nicer points illustrated with appropriate drawings *.

Section V.—*Apparitions from, or from behind the Retina; with Corollaries.*

The next object for study behind those in the vitreous, are the vasa centralia retinæ, which are imbedded in the substance of the

* The 36th vol. pp. 97–104, of the Lond. Med. Gazette, is quoted to show that the writer maintained in 1845 that the usual muscæ volitantes are but apparitions of portions of the essential structure of the vitreous body, and that he then fundamentally and clearly enunciated the view now more particularly developed. Other writers have regarded these as remnants of the fœtal eye, or as pathological fragments, floating freely, or in loculi of the vitreous humour; for the most part differing very widely from the reasonings and conclusions in this essay. The writings of Brewster, Donders, Doncan, &c., are referred to.

membrane in such a manner as to penetrate from the side of the hyaloid membrane outwards from the eye's centre, deeper as they approach the punctum aureum. They may be called into view by any pencil of rays that is in the act of sweeping over the retina. In the well-known experiment of Purkinje, of waving a candle-flame before the eye, the radiating point is the image of the flame at the back of the eye. In this experiment the vessels display a remarkable parallactic gliding over the visual field, first observed by Gudden, in 1849; but it was left for H. Müller, a few years after, to point out the cause of the phenomenon, and to calculate from entoptical observations the distance of the vessels from the "perceiving membrane" lying without them, required to account for the parallax. This essay adopts his hypothesis, but supports it by independent observations, and substitutes another mode of calculating the said distance.

The flame is made to pause successively, on opposite sides of the vessel to be observed, in an ocular meridional plane, or that of some great circle, so that the shadow may be seen to deviate equally, twice in one plane, from the retinal radius that passes through it, and this whole angle, β, is noted, as well as that, α, between the two positions of the flame, as viewed from the eye's centre. Then, if d be the perpendicular distance of the vessel from a sentient surface whose radius is r, it is found that

$$d=r\left\{1-\frac{\cos\frac{1}{4}(\alpha+\beta)}{\cos\frac{1}{4}(\alpha-\beta)}\right\}.$$

If we imagine a dark spot without the sentient surface, as the pigment of the choroid, visible through the retina at the foramen centrale, to simulate a shadow by being seen, through a second reflexion of the rays already radiating by reflexion at the flame's image on the eye's coats, by a sentient surface lying *within* it; then if d be the distance of the dark spot from such a surface, we shall have

$$d=r\left\{\frac{\cos\left(\frac{1}{4}\alpha-\frac{\beta}{2}\right)}{\cos\frac{1}{4}\alpha}-1\right\}.$$

If a vessel imbedded in the sentient surface were to have its

shadow thrown back upon that surface, through reflexion from some tunic at the distance d without it, we should have

$$d=r\left\{1-\frac{\cos\frac{1}{4}\left(\alpha+\frac{\beta}{2}\right)}{\cos\frac{1}{4}\left(\alpha-\frac{\beta}{2}\right)}\right\}.$$

And some slight modification of this equation would meet the case of a parallactic deviation by a second reflexion, should a vessel lie either a little without or within the sentient surface.

The parallaxes here suggested would all take place in the direction of those actually witnessed in Purkinje's experiment, and the calculated values of d would so far approach each other from the same values of α and β (since β is small), that we ought not to rest satisfied with the fact of these values agreeing well with the simple conception of the sentient surface lying a little without the vascular plexus; especially as there are two supplementary versions of the vascular phantom rendered manifest by the experiment, one very notable one to which attention is directed—and that were the shadows of the vessels displayed by a second reflexion, as imagined in the third equation, there would be more than one version of them; as, moreover, a dark figure of the foramen centrale is rendered visible at the same time. However, these other versions of the figure are eliminated by being traced to other sources, and with H. Müller, the central dark spot is treated as the shadow of the wall of the foramen centrale; so that the sentient surface must be without the brim of the foramen.

The essay, with as much of method as is available, now passes on in quest of the causes of other spectral phenomena, in the production of which light is not an agent. It cites Young's observation of the images of objects that press from the outside of the eye upon the sclerotic coat, being seen by *flexure* of the retina along their outline; notices, as of this type, the circles of light seen at the bases of the optic nerves on turning the eyes sharply in their sockets; touches upon the colours displayed in such experiments; and points out how that wherever the retina is so compressed as to evince quasi-lights, it comparatively or entirely fails to render us acquainted with the existence of luminous objects. It then explains how the retina is flexed and compressed by the action of the orbital muscles, always,

to some extent, when we fix the eye's axes in a given direction; and severely, whenever we wilfully strain our vision—thus astonishing us by the flitting away of objects from our sight, burying some in quasi-lucid clouds, as if they had overspread one another, and as the origin of the phenomenon was undetected, occasioning many surmises upon the inherent qualities of the special nervous structure in order to account for them. An observation upon the inverted image of a candle formed at the posterior face of the crystalline lens is mentioned, which indicates other muscular action besides that which rotates the eyeball, when the eye is vehemently strained, as if the lens becomes flattened. The phenomena which inform us of a differential structure in the retinal surface, with respect to the punctum cæcum, foramen centrale, and the elementary rods and cones, which H. Müller believes to constitute the sentient layer, are adduced; as well as the conclusions to which we are led, after eliminating the various phenomena studied, as regards the ultimate structure of the sentient surface.

VIII. "On Hourly Observations of the Magnetic Declination, made by Captain Rochfort Maguire, R.N., and the Officers of H. M. Ship 'Plover,' in 1852, 1853 and 1854, at Point Barrow, on the shores of the Polar Sea." By Major-General Edward Sabine, R.A., D.C.L., Treas. and Vice-President R.S. Received August 14, 1857.

(Abstract.)

Point Barrow is the most northern cape of that part of the American continent which lies between Behring Strait and the Mackenzie River. It was the station, from the summer of 1852 to the summer of 1854, of H.M.S. 'Plover,' furnished with supplies of provisions, &c. for Sir John Franklin's ships, or for their crews, had they succeeded in making their way through the land-locked and ice-encumbered channel by which they sought to effect a passage from the Atlantic to the Pacific. In this most dreary, and apparently uninteresting abode, Captain Maguire and his officers happily found an occupation in observing and recording, for seventeen months unremittingly, the hourly variations of the magnetic declination and of the

concomitant auroral phenomena, in a locality which is perhaps one of the most important on the globe for such investigations. Their observatory, placed on the sand of the sea-shore, was constructed of slabs of ice, and was lined throughout with seal skins. The instruments had been supplied from the Woolwich establishment, with the requisite instructions for their use, and the observations were made and recorded precisely in the same manner as those in the Colonial Magnetic Observatories. The observations were sent by Captain Maguire to the Admiralty, and were in due course transmitted to General Sabine, by whom they were subjected to the same processes of reduction as those in the colonial observatories: the results are given and discussed in this communication.

A sufficient body of the larger disturbances to permit an examination of their laws having been separated from the rest of the observations, it was found at Point Barrow, as elsewhere wherever a similar investigation has been made, that in regard to the frequency of their occurrence, and to the mean amounts of easterly and westerly deflection produced by them, the disturbances follow systematic laws depending on the hours of solar time. The laws of the easterly and of the westerly disturbances were also found, at Point Barrow as elsewhere, to be distinct and dissimilar. On further instituting a comparison between the disturbance-laws at Point Barrow and Toronto, it was found that although the laws of the deflections of the same name at the two stations did not correspond, there existed, on the other hand, a very striking and remarkable correspondence between the laws of the easterly disturbances at Point Barrow and of the westerly at Toronto, and between the laws of the westerly disturbances at Point Barrow and easterly at Toronto. The correspondence is traced in much detail, for the purpose of showing that it is manifested, not in slight and unimportant particulars, but in the most marked characteristics of both classes of phenomena. From the correspondence in the hours at which opposite disturbance-deflections prevail, it follows, that the portion of the diurnal variation which depends upon the disturbances, has opposite, or nearly opposite characteristics at the two stations.

In former papers the author considers that he has shown that, for the purpose of obtaining a correct knowledge of the phenomena of the *regular solar diurnal* variation, it is necessary to eliminate the

influence of that portion of the observed diurnal variation which proceeds from the *disturbances*; and he now adduces the observations at Point Barrow as strongly confirmatory of this. When the diurnal variation is derived from the whole body of the observations at Point Barrow, retaining the disturbances, the westerly extreme of the diurnal excursion, which, as is well known, occurs generally in the extra-tropical part of the northern hemisphere at a little after 1 P.M., is found to take place at 11 P.M.; but when the larger disturbances are omitted, the westerly extreme falls at the same hour as elsewhere, viz. a little after 1 P.M. The author takes occasion to suggest the probability that the anomalies which have been supposed to exist elsewhere in the turning-hours of the solar diurnal variation in high latitudes may be susceptible of a similar explanation.

It appears, therefore, by the comparison of the Point Barrow and Toronto observations, that in the regular solar diurnal variation the progression at the two stations is similar, the easterly and westerly extremes being reached nearly at the same hours; whilst in the disturbance diurnal variation the progression is reversed, the easterly extreme at the one station coinciding very nearly with the westerly extreme at the other. This contrariety seems the more remarkable, since both variations appear to have the same primary or exciting cause, viz. the sun; inasmuch as in each the period is a solar day. The author draws the probable inference, that whilst the primary cause is the same in both, the mode of operation is different in the two cases.

Another important distinction between the phenomena of the solar diurnal variation and of the disturbance variation at Point Barrow and Toronto, is shown by the author to consist in the comparative magnitude of their range. The increase in the range of the solar diurnal variation between Toronto and Point Barrow is, as nearly as may be, in the inverse ratio of the values of the horizontal force of the earth at the two stations (which is the antagonistic force opposing all magnetic variations); whilst on the other hand the increase in the range of the disturbance variation is many times greater than it would be according to the same proportion. It would appear therefore that the absolute disturbing force must be much greater at Point Barrow than at Toronto; suggesting the question, by what physical

or other conditions is the locality distinguished at which the disturbing force is a maximum.

In correspondence with the great amount of the absolute disturbing force at Point Barrow is the frequency of the concomitant auroral manifestations, which greatly exceed that of any previous record known to the author. It was the custom at Point Barrow to attach a distinguishing mark to all the hourly magnetic observations which were made when the Aurora was visible. Taking the months of December, January, and February as those in which, in the latitude of Point Barrow, there is nearly a constant absence of day-light, there were, in those three months in 1852–1853, 1788 hourly observations, at 461 of which the Aurora was visible; and, in the same three months of 1853–1854, there were 1837 hourly observations, at 616 of which the Aurora was seen. There were therefore in the six months 3625 hourly observations, at 1077 of which the Aurora was seen contemporaneously, and at 2548 it was either not present or obscured by clouds: the presence of the Aurora was thus recorded at between one-third and one-fourth of the hours of observation. Or possibly the frequency of its appearance may be more easily judged of by stating, that during these six months the Aurora was seen on six days out of every seven.

The record thus furnished by Capt. Maguire has enabled the author to treat the Aurora, for the first time, in the same way as the corresponding and connected phenomena of the magnetic disturbances are treated, viz. by distributing its recorded appearances into the several hours of their occurrence. A table, in which the particulars of this distribution are contained, shows that 1 A.M. is the hour of their most frequent occurrence at Point Barrow, there having been 102 recorded appearances in the six months at that hour. From 11 A.M. to 3 P.M. is the epoch of minimum, not a single instance of Aurora at any of those hours being recorded in the same six months. The increase from the minimum to the maximum, and the decrease from the maximum to the minimum, are both continuous progressions, with only such very slight occasional interruptions as might assuredly be expected to disappear in mean numbers taken from a longer interval of time.

When the frequency of the Aurora at the different hours is compared with the respective amounts of easterly and of westerly disturb-

ance-deflection at the different hours at Point Barrow, a very considerable approximation towards accordance is perceived between the frequency of the Aurora and the amount of the *westerly* deflections; whilst, on the other hand, the auroral hours appear to have little or nothing in common with the turning-hours, or with the progression, of the *easterly* deflections.

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END OF THE EIGHTH VOLUME.

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